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Source of Community Leaders Series

George M. Foster

AN ANTHROPOLOGIST'S LIFE IN THE TWENTIETH CENTURY: THEORY AND PRACTICE
AT UC BERKELEY, THE SMITHSONIAN, IN MEXICO, AND WITH THE
WORLD HEALTH ORGANIZATION

With an Introduction by
E. A. Hammel

Interviews Conducted by
Suzanne Riess
in 1998 and 1999

Since 1954 the Regional Oral History Office has been interviewing leading participants in or well-placed witnesses to major events in the development of northern California, the West, and the nation. Oral history is a method of collecting historical information through tape-recorded interviews between a narrator with firsthand knowledge of historically significant events and a well-informed interviewer, with the goal of preserving substantive additions to the historical record. The tape recording is transcribed, lightly edited for continuity and clarity, and reviewed by the interviewee. The corrected manuscript is indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library at the University of California, Berkeley, and in other research collections for scholarly use. Because it is primary material, oral history is not intended to present the final, verified, or complete narrative of events. It is a spoken account, offered by the interviewee in response to questioning, and as such it is reflective, partisan, deeply involved, and irreplaceable.

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George M. Foster, 1998.

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Anthropologist

An Anthropologist's Life in the 20th Century: Theory and Practice at UC Berkeley, the Smithsonian, in Mexico, and with the World Health Organization, 2000, xii, 413 pp.

Family and background, Ottumwa, Iowa; anthropology at Northwestern, Melville Herskovits; Ph.D. at UC Berkeley, Alfred Kroeber, Robert Lowie; first travel to Mexico; marriage to Mary LeCron, 1938, and trip to Austria; research with Sierra Popoluca, 1940-1941; teaching at Syracuse and UCLA; colleagues and work at Smithsonian Institution, Washington and Mexico: Institute of Inter-American Affairs, Institute of Social Anthropology, 1943-1953, start of long-term field research in Tzintzuntzan, sabbatical in Spain; UC Berkeley Department of Anthropology since 1953: planning Kroeber Hall, course work, administration, expanding faculty, Ph.D. curricula, funding students; American Anthropological Association presidency; sixties, seventies issues of free speech, ethics, Vietnam war; evolution of medical anthropology; community development advisory role for World Health Organization, Agency for International Development; discusses field work, writing, students, personal change, beliefs, family, friendships, and some current issues in anthropology.

Introduction by Eugene A. Hammel, Professor of Anthropology, Emeritus, UC Berkeley.

Interviewed 1998-1999 by Suzanne B. Riess for the Sources of Community Leaders Oral History Series, Regional Oral History Office, The Bancroft Library, University of California, Berkeley.

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PREFACE

On the occasion of the 50th anniversary of our graduation from the University of California at Berkeley, the Class of 1931 made the decision to present its alma mater with an endowment for an oral history series to be titled "The University of California, Source of Community Leaders." The Class of 1931 Oral History Endowment provides a permanent source of funding for an ongoing series of interviews by the Regional Oral History Office of The Bancroft Library.

The commitment of the endowment is to carry out interviews with persons related to the University who have made outstanding contributions to the community, by which is meant the state or the nation, or to a particular field of endeavor. The memoirists, selected by a committee set up by the class, are to come from Cal alumni, faculty, and administrators. The men and women chosen will comprise an historic honor list in the rolls of the University.

To have the ability to make a major educational endowment is a privilege enjoyed by only a few individuals. Where a group joins together in a spirit of gratitude and admiration for their alma mater, dedicating their gift to one cause, they can affect the history of that institution greatly.

The oral histories illustrate the strength and skills the University of California has given to its sons and daughters, and the diversity of ways that they have passed those gifts on to the wider community. We envision a lengthening list of University-inspired community leaders whose accounts, preserved in this University of California, Source of Community Leaders Series, will serve to guide students and scholars in the decades to come.

Lois L. Swabel
President, Class of 1931

William H. Holabird
President, retired, Class of 1931

Harold Kay, M.D.,
Chairman, Class of 1931 Gift Committee

September 1993
Walnut Creek, California

UNIVERSITY OF CALIFORNIA, SOURCE OF COMMUNITY LEADERS SERIES

July 2000

Robert Gordon Sproul Oral History Project. Two volumes, 1986.

Includes interviews with thirty-four persons who knew him well:
 Horace M. Albright, Stuart LeRoy Anderson, Katherine Connick
 Bradley, Franklin M. "Dyke" Brown, Ernest H. Burness, Natalie Cohen,
 Paul A. Dodd, May Dornin, Richard E. Erickson, Walter S. Frederick,
 David Pierpont Gardner, Marion Sproul Goodin, Vernon L. Goodin,
 Louis H. Heilbron, Robert S. Johnson, Clark Kerr, Adrian A. Kragen,
 Mary Blumer Lawrence, Stanley E. McCaffrey, Dean McHenry, Donald H.
 McLaughlin, Kendric Morrish, Marian Morrish, William Penn Mott, Jr.,
 Herman Phleger, John B. deC.M. Saunders, Carl W. Sharsmith, John A.
 Sproul, Robert Gordon Sproul, Jr., Wallace Sterling, Wakefield
 Taylor, Robert M. Underhill, Eleanor L. Van Horn, Garff B. Wilson,
 and Pete Yzaguirre.

Bennett, Mary Woods, class of '31, A Career in Higher Education: Mills
 College 1935-1974, 1987.

Bridges, Robert, class of '30, Sixty Years of Legal Advice to
 International Construction Firms; Thelen, Marrin, Johnson and
 Bridges, 1933-1997, 1998.

Browne, Alan K., class of '31, "Mr. Municipal Bond": Bond Investment
 Management, Bank of America, 1929-1971, 1990.

Coliver, Edith, class of '43, foreign aid specialist (in process).

Dettner, Anne DeGruchy Low-Beer, class of '26, A Woman's Place in Science
 and Public Affairs: 1932-1973, 1996.

Devlin, Marion, class of '31, Women's News Editor: Vallejo Times-Herald,
 1931-1978, 1991.

Foster, George M., class of '35, Ph.D. '41, An Anthropologist's Life in
 the Twentieth Century: Theory and Practice at UC Berkeley, the
 Smithsonian, in Mexico, and with the World Health Organization,
 2000.

Foster, Mary LeCron, Ph.D. '65, linguist on symbolism in culture and
 language (in process).

Hassard, H. Howard, class of '31, The California Medical Association,
 Medical Insurance, and the Law, 1935-1992, 1993.

- Hedgpeth, Joel W., class of '33, Marine Biologist and Environmentalist: Pycnogonids, Progress, and Preserving Bays, Salmon, and Other Living Things, 1996.
- Heilbron, Louis H., class of '28, Most of a Century: Law and Public Service, 1930s to 1990s, 1995.
- Hoadley, Walter, M.A. '38, Ph.D. '46, Business Economist, Federal Reserve System Director, and University of California Regent, 1938-2000, 2000.
- Kay, Harold, M.D., class of '31, A Berkeley Boy's Service to the Medical Community of Alameda County, 1935-1994, 1994.
- Kittredge, Janice, class of '47, environmentalist (in process).
- Koshland, Daniel E., Jr., class of '41, professor of microbiology (in process).
- Kragen, Adrian A., class of '31, A Law Professor's Career: Teaching, Private Practice, and Legislative Representative, 1934 to 1989, 1991.
- Lin, T. Y., M.S. '33, professor of structural engineering (in process).
- Peterson, Rudolph A., class of '25, A Career in International Banking with the Bank of America, 1936-1970, and the United Nations Development Program, 1971-1975, 1994.
- Schwabacher, James H., Jr., class of '41, music teacher and patron of arts (in process).
- Stripp, Fred S., Jr., class of '32, University Debate Coach, Berkeley Civic Leader, and Pastor, 1990.
- Torre, Gary, class of '41, Labor and Tax Attorney, 1949-1982; Sierra Club Foundation Trustee, 1968-1981, 1994-1998, 1999.
- Trefethen, Eugene E., class of '30, Kaiser Industries, Trefethen Vineyards, the University of California, and Mills College, 1926-1994, 1997.

INTRODUCTION by E. A. Hammel

Foster returned to Berkeley as a faculty member in 1953, while I was still in military service. On my return in 1955 he was director of the UCB Museum of Anthropology, where I took up my old job as a preparator and photographer. The museum was at that time still in the old Civil Engineering building, where Campbell Hall now stands. Old-timers like me (I had worked in the museum 1947-51) found Foster much stricter, much more focussed on efficiency than Gifford. There was some grumbling among us at these reforms, and consternation when at one point Foster (doubtless in agreement with Heizer) decided that totally undocumented artifacts (including some tons of California rock mortars and heaps of cow bones from some Mission sites), useless for any plausible scientific purpose, should be laid to rest at the bottom of San Francisco Bay. He was fair, but there was just no nonsense.

Foster was active in shaping the graduate curriculum, and his penchant for efficiency and reform showed there, as well. The practice in the department, at least since his own time as a student and before, had been to hold an early written preliminary examination of a day's duration in the first or second year (the "baby prelims"). That was followed perhaps two years later by the written comprehensive exam (two days), and if one passed, the oral qualifying exam, and then the "Ten Day Problem". Students who passed the orals were handed a research assignment on their way out the door, a problem that they had to complete in the form of a formal written report within ten days. Foster, with other colleagues, eliminated the Ten Day Problem; I was the first student to benefit. He, and they, also streamlined the examination process, so that the baby prelims were eliminated, and he played a role in the eventual elimination of all of the written preliminary examinations at a much later date. Foster was a pretty tough examiner. I remember, as part of my duties as the department factotum (a part time job I also held) being told to file the examination papers for the baby prelims. Written crisply across the front of mine, in Foster's hand, was the notation, "Somebody has to teach Hammel to write." Perhaps it was the experience of reading my exams that led him and his colleagues to eliminate the writtens!

It is important to note that in all of his curricular reform efforts, especially after 1958, when he became Graduate Advisor, Foster consulted extensively with the graduate students. In those days he lived on Kentucky Avenue, and I can remember meetings there, with Baumhoff, Romano, and other senior grad students, Foster asking probing questions, what to keep, what to jettison, how to make the curriculum relevant to the eventual tasks of research. Foster was the architect of the modern Ph.D. program at Berkeley, especially of the tracking of student progress. Of course, he had to carry all of those ideas to his

faculty colleagues, but I think he (and also Rowe) were prime forces in reform. In all of this there came through the same dedication to institutional construction and success that he later showed in the difficult task of managing the department as chairman. When Ed Norbeck was hospitalized with a bleeding ulcer I remember the stricken look on Foster's face as he told me the news. He had two concerns. The first was for Norbeck. The second was for the structure of the curriculum; who would teach those courses? This characteristic showed again some time later. Walking from Kroeber Hall toward Piedmont one day, Foster shared his thoughts with me, a new faculty member--about the age structure of the department, about how more younger people were needed, to teach new subjects, to keep the curriculum in touch with new developments in the field. His mind was always on making Berkeley a great department.

Foster was instrumental in developing the plans for Kroeber Hall in the 1950s, especially concerning the inclusion of the museum. He pored over the architect's drawings, detail by detail. He did it with much attention to broad advice. I remember as a student and the museum photographer, drawing at his request rough sketches for the museum darkroom and was amazed, on my return some years later, to find them realized in real structures. He pushed aggressively on the Kroeber Hall project, but he solicited the suggestions of all the staff. I can remember his discussion of the original office layouts on the third floor, with a very large room at the head of the main stairs. He had age structure in mind again; this was to be a "bullpen" that could be shared by emeriti twenty years down the road. Of course, senior faculty were all "bulls" in those days.

When I returned from Peru in September 1958 to write my dissertation, Foster was the second reader. I think I never really understood him until that experience. I had written a draft of the dissertation in my father-in-law's garage in Delaware, and brought it with me to Berkeley. I gave it to Foster one Friday. On Monday he called me in, handed it back, and told me to start over, giving me a list of suggestions. One of them was to "bring the readers into the Valley on a bus, give them a feel for it." I spent a solid week rewriting and gave the revisions to Foster on a Friday. On Monday he called me in ..., and this process was repeated several times, monopolizing his weekends. What is most important about this anecdote is its illustration of Foster's complete dedication to his task, at the expense of his own interest, and the promptness of his response.

He was surely instrumental, along with Rowe, in clearing the path to my first teaching job at UNM in 1959, through his connections with Leslie Spier. He was also instrumental, as chairman (but doubtless also with Rowe), in constructing the invitation to me to return to Berkeley in 1961. We had kept in touch, he had visited Albuquerque, and he seems to have played his card when he saw Gordon Willey's hand and an

invitation to Harvard. That was pure Foster--aware of what was going on in the discipline, thinking about the structure of the Berkeley program, raising the bets when needed. It was in those years that the Fosters moved from Kentucky Avenue to San Luis Road, explicitly building a house in which they could entertain the members of the department, doing this with grace, and signalling the way in which they would, years later, again use their resources to its benefit.

By the time I returned to Berkeley in 1961, several of the leading social anthropologists (Schneider, Fallers, Geertz) had left, and there was considerable tension between some of the faculty remaining over the department's failure to retain them. This tension was exacerbated by Washburn's development of primatological approaches to society and culture, which were seen as intrusive by some faculty (notably Murphy, who had been "left behind" by the departees), and by Foster's own managerial efforts in retention and recruitment. In those days, personnel matters at Berkeley had rested exclusively with the tenured faculty. As chairman, Foster brought the junior faculty into at least the preliminary discussions, if not the vote. It was not until then that I realized the intensity of such processes and the acrimony that could be generated. My young colleagues and I were very uncomfortable under those circumstances, thrust into debate with elders who would ultimately decide our own fates. It was much riskier than participating as a graduate student in discussions of the curriculum. Foster started both of these trends, first as graduate advisor, then as chairman. Indeed, in retrospect, it was Foster who began the democratization of the department, well in advance of broader moves in those directions in the university at large. Nevertheless, he was often seen by younger faculty and students as old-fashioned and certainly not ideologically correct. Reformers are usually so regarded by revolutionaries.

Nader and I have already written on Foster's willingness to make changes and take risks in his intellectual approaches, and I do not touch on those points here. Instead, I concentrate on the political drama. The events of the early sixties had developed an important schism in the department, mostly ideological but partly generational. It was deepened by the events of the middle sixties at Berkeley but was not to reach its peak until the last years of the decade, when many faculty were torn not only by conflicting loyalties to their ideological convictions, to their institutions, to one another, but also pulled by loyalty to their students, most of whom were actively protesting elite hegemony and especially the war in Southeast Asia. Throughout these years, Foster struggled to help his students, trying to support them but reasoning conservatively with those who were participating in sit-ins (not to mention cooling their heels in jail), trying to preserve institutional structures when many of them (and some of his colleagues) just wanted to blow them up.

At the end of the decade, in his term as president of the American Anthropological Association, Foster was caught in the vortex. Several distinguished anthropologists, some of them former Berkeley colleagues, were leaders in the opposition. Eric Wolf had taken a leading role in the antiwar movement. David Aberle was on the executive board, had deep sympathies with that movement, and his wife (Kathleen Gough) had been extremely active in it. David Schneider, one of the departees from Berkeley, and a man whose personal relations with Foster had never been particularly good, was sympathetic to the movement and was also on the board. I had been elected to the board in the same year Foster was elected president-elect. Foster tried hard to keep the association on a scholarly and professional track. I was close to Foster, but also to Schneider, and Schneider to Aberle, and Aberle to the activists. A kind of shuttle diplomacy developed along that chain, resulting in some not unreasonable compromises that allowed the association to function. But Foster was drained by this experience, discouraged by having militants paint swastikas on his office door in Berkeley, and wearied by the ongoing administrative problems of the association. He had been instrumental in getting the association back on its feet, fiscally and organizationally, after a period of disastrous mismanagement. Unlike most academics, Foster knew how to read a balance sheet. He was in many ways responsible for its survival, not to mention its expanded professional role. I can remember his frustration when, at a board meeting, the executive director of the association (a very competent person who truly advanced its interests), seeking more status and recognition, locked himself in his office in a complete snit. I recall Foster standing outside that door, saying plaintively, "Please come out, Ed." All the while, the activists on the board wanted him to pay more attention to Vietnam.

After those years, as the university healed (even though the department went on to new ideological schisms and departures), things cooled down for Foster. We saw less of each other, as my interests turned to analytic issues that were different from his own, and as my administrative and teaching duties outside the department increased. In retrospect I can see that it was in that period, through the eighties and nineties, that Foster returned to his true task, the support and building of his beloved (but sometimes ungrateful) department. Stubbornly, with an eye on the future and perhaps on the most distant past when the Berkeley department under Kroeber was one of the most eminent in the world, disregarding the wounds of the immediate past, the Fosters began to endow the department. It was very quietly done. The first of these acts that I remember was when the Gifford Room (the departmental lounge) was inexplicably furnished with lovely chairs and couches. At a departmental meeting one day, Foster came in and sat down on one of the couches. The frame broke, and Foster jumped up in angry and clearly proprietary alarm. No one let on, but the whole department chuckled for a week. Especially in recent years, the Fosters have laid a basis for the department's institutional independence.

That's the Foster that I remember. Minding the business of the department, struggling between strictness and compassion, stubborn in his ideas, conservative, a believer in rules but adept in using them to advance the interests of students and the department, generous to a fault. You don't have to agree with him (and I often didn't). You don't have to like him (and some didn't). But you had to admire and respect him as one of the great builders in the history of the department and the discipline, more concerned about the success of institutions than about his own--sadly a rare trait in commonly egomaniac academia. It amazes me that Foster expresses no ill will toward colleagues who, literally, once tried to destroy him. His capacity for forgiveness is quite large. In their loyalty to the discipline, to the department, and to each other, George and Mickie remind me much of their predecessors in the museum, the Giffords--an island of stability in an uncertain sea.

E. A. Hammel
Professor of Anthropology, Emeritus
University of California, Berkeley

Berkeley, California
June 2000

INTERVIEW HISTORY--George Foster

George M. Foster, anthropologist, professor at Berkeley from 1953 to 1979, has worked in peasant economics, material culture, folklore, symbolism, linguistics, role analysis, community development, and public health. His field work and consultancies have taken him to countries ranging from Afghanistan to Peru. The bibliography appended testifies to the consistency of his publication. Eugene Hammel and Laura Nadar, anthropology department colleagues, wrote of Foster in 1996:

Perhaps even more impressive than the honored status of his individual works is the integrity of the full corpus of Foster's scientific, academic, pedagogical, and professional contributions. His career is surprisingly of a piece; nothing in this record is really isolated. For example, his work on pottery is a part of his interest in the economy, which blends with that on community development and public health, and his interest in community development (and his perception that peasants often lack such an interest) sparked investigations of role behavior, worldview, and culture change. All of his research experience, professional devotion, and productivity have been brought to his teaching and to his administrative responsibilities in the Department and University in which he has spent the last twenty-five years. His own intellectual development is an object lesson, and his influence on students in social science has been and continues to be immeasurable.¹

The following oral history with George Foster ambitiously aims to tell the reader as much as possible about the life of this man, his family origins, the belief systems he grew up with, the choices he made about education, the adventures and opportunities he found around the world, the way he learned to question what he was seeing and hearing, the pen to paper integration of years of keen observations, and his home and family and friends.

Oral history gathering is in some ways akin to anthropology, and it is with a happy sense of appropriateness that the Regional Oral History Office has launched a series of interviews with members of UC Berkeley's Department of Anthropology. While the main subject of the interviews will always be the individual's life and work, the focus will be wider because this is a time in the history of the anthropology when interviewees can both look back and reflect on, for instance, the

¹"Will the Real George Foster Please Stand Up? A Brief Intellectual History." By E. Hammel and L. Nader. <http://sunsite.berkeley.edu/>

seminal teachings of Boas, of Kroeber and Lowie and Herskovitz and Mead, and also look around and forward and make enlightened comment on changes in the field.

I first met George Foster through the University Anthropology Department website <http://sunsite.berkeley.edu/Anthro/foster/index.html>. There he was pictured, a tall man in a hat with a big bag slung over his shoulder, photographed with what I assumed were Mexican peasants in the shade of a log-columned portico. The website was created around the Fifth Emeritus Faculty Lecture honoring George M. Foster, and it sang his praises. The next meeting, real time, was on San Luis Road, in the Fosters' handsome Bay-facing cliff-hanging house. George Foster and his anthropologist wife Mary "Mickie" LeCron Foster sat with me at a table adjacent to their lofty kitchen. I was aware of great stuff around me, excellent pottery, fabrics, paintings, books, but I was preoccupied with explaining how small my knowledge of anthropology was. I was demonstrating appropriate modesty in the face of two lifetime achievers.

A letter of invitation to be an oral history memoirist had already come to George Foster from the Regional Oral History Office. He had accepted, and signed, the die had been cast, and he was eager to begin the interviews. But then there was the question of just when to begin, as the Fosters had a lengthy adventure cruise lined up. George and I thought "future," after the new year, 1999. Mickie challenged us with "now." And now it was, we began the next week, on November 11, 1998.

The interviews followed a chronological course, with a syllabus of subjects kept more or less firmly in mind. Before the interviews began George Foster loaned me a videotape with him made in 1989 by Charles Wagley for the Human Studies Film Archives Dialogues in Anthropology. I watched it. In it George narrated early horseback adventures in Mexico, in search of his first field subjects, the Popoluca of Veracruz, as well as alluded to later distressing adventures with the American Anthropological Association. He seemed to me on the video to be awfully stiff, and it was a great pleasure to discover that George Foster, at his kitchen table, with a life to roll out to the tape-recorder before him, was not at all stiff.

Rather, he was totally attentive, really thinking of the subject at hand, showing such a fine application of mind, start to finish. No detail of the oral history process got less than his full attention, from interviewing to editing to a final review. George Foster rose early and went to work in his office at home every day. He had complex writing projects, as well as rather stunning health problems, as a constant backdrop to the oral history interviews, but he was able to concentrate his mind on the subject at hand. Oral history was fortunate with George Foster.

We met weekly, around 9:00 in the morning, picking up where we had left off. Sometimes there would be a particular publication assigned me to read to prepare for the next session, or to discuss to elucidate the last. More usually my work was to review his writings as they came along chronologically in the story. The tape-auditing and editing that I did, both ongoing and at the completion of the interviews, were then followed by a complex process of further editing for George Foster. The Parkinson's-like disease from which he suffered made handwriting too difficult for noting the changes in wording and corrections that were necessary in the long transcript. To solve this problem we tried a variety of systems, from his reading to me what he wished to say, or to have said, where marked with a tick on his copy of the transcript, and then my scribbling madly on my copy, to his final efficient and sophisticated solution of typing the changed phrases into his computer, with the page and line noted, printing it out, and giving it to me for discussion and insertion.

Such editing took a lot of time for both of us, but it was in every way clear that George's oral history was more than flinging assorted recollections into the archives; it was an autobiography, and there was pride at stake and importance in doing it right. In that way George and I were definitely a team. I have not done anthropological fieldwork, but I certainly got a good idea in the course of the interviews of how George was always a teacher, in the field and at home, and at heart a writer--he speaks in the oral history of important lessons in writing from Alfred Kroeber. I was confronted with a quantity of what teachers call "careless errors" in editing and proofing that would have resulted in an alarming "grade" from Professor Foster if that were the relationship. But our autobiographical team worked just fine, and with mutual affection.

If the exchange of food stands for a larger sharing and cementing of a relationship, I offer two examples. I made several trips to Pennsylvania to visit my mother, in her last months of life in a nursing home, and I came back from one of those trips with slabs of scrapple, a pig-product of which Pennsylvanians from my part of that state eventually grow very fond, fried until crisp, served with applebutter. I wanted very much to fry up and serve this treat to George, who comes from the pig-product background of Ottumwa, Iowa, from Morrell Packing Company people--indeed, as you will read, if he hadn't clearly and intentionally escaped it, pork could have been his life's work. And I made applebutter from my Berkeley apple trees.

I was ready to demonstrate my skills, nothing to do with oral history. But that little feast was thwarted by a large Foster family visit and never reinitiated. However, one day when I arrived for the early morning interview, and was packing up to leave close to noon--George had already departed for one of his several regular collegial lunches--I was sorely tempted by a peanut butter cookie in a glassine

envelope, lying on the kitchen counter. I confessed to my temptation the next time we met, and from then on I was to receive occasional packages of these great big peanutty cookies, secured from a stand along the road the Fosters took to their Calaveras County country place. Shared treats.

I am grateful to George Foster for his attention to detail, and for demonstrating throughout the process graciousness and well-bred charm. His firm grip on the tiller is in evidence. I am grateful to Eugene Hammel for quickly taking on the honor and challenge of writing yet another introduction to his friend and colleague, George Foster, an introduction that touches importantly on several issues that were struggles in George's career. Suzanne Calpestri, head of the Anthropology Library, gave the whole project an important push at the outset. James R. K. Kantor provided vital proofreading of the manuscript. Willa Baum, division head of the Regional Oral History Office, has always firmly believed that we must "get going" on anthropology, and now we are auspiciously launched. George Foster, avid ships, trains and planes buff that he is, will approve such vehicular mixed metaphors!

The Regional Oral History Office was established in 1954 to augment through tape-recorded memoirs the Library's materials on the history of California and the West. Copies of all interviews are available for research use in The Bancroft Library and in the UCLA Department of Special Collections. The office is under the direction of Willa K. Baum, Division Head, and the administrative direction of Charles B. Faulhaber, The James D. Hart Director of The Bancroft Library, University of California, Berkeley.

Suzanne B. Riess, Interviewer/Editor

July 2000
Regional Oral History Office
The Bancroft Library
University of California, Berkeley

I FAMILY AND BACKGROUND

[Interview 1: November 12, 1998] ##¹

Grandfather, Thomas Dove Foster

Riess: You were born in Sioux Falls, South Dakota, in 1913. I'd be interested in how your family had gotten there--your parents and perhaps grandparents and their country of origin.

Foster: I'll begin with my grandfather [Thomas Dove] Foster, who was born in Bradford, England in 1847. His father, William, had married Mary Morrell, a member of a family of provisioners that later became John Morrell & Co., a major meat packer in this country. It was a family business until about 1960. I was expected to go into the business, but fortunately I didn't.

My grandfather, who died in 1915, I never knew. I'm told that he looked at me once and approved of me, but I have no recollection of the event. In 1868 he made his first trip to North America, on the Inman liner *City of Paris*, arriving in New York in late July. He was sent by his family to the New World to find sources of pork for the British market. In 1859, when he was fourteen years old, he had moved with his parents to Castlecomer, in Kilkenny County, where he lived until 1865, completing such schooling as he had and, working in the meat business, learning the trade techniques and needs. Ireland provided a good deal of the meat of Britain in the middle of the last century, I'm told. But that was not sufficient, so he came first to Canada, Ontario, after a brief stay in New York, and then in 1871 he was in Chicago at the time of the great fire.

He wrote a very interesting letter home to his parents, which survived and has been published privately by the family,

¹## This symbol indicates that a tape or tape segment has begun or ended. A guide to the tapes follows the transcript.

telling of his experiences in the fire. A number of things interested me in this. One was, he carried a gun at the time, which I take to be totally out of keeping with everything I knew about the family, and he used his gun in persuading a drayman to carry his trunk out of the fire.

Riess: He thought America was a wilderness?

Foster: I have no idea where or why he acquired it. He just said that, "I pulled out my gun and persuaded the drayman to take me to the edge of the city."

His first wife was named Eliza Thompson. They were married in Brooklyn in 1872. She had been born in Ireland, and had come to America with her parents and brothers as a young girl, and the family had settled in Brooklyn. There were four children in that family. The eldest was William Heber Thompson Foster. The second was Thomas Henry Foster. There were two sisters: Mary, who grew to maturity but died before I ever knew her. She married one of the Hormels, and I don't think they had any children. The fourth of that marriage was my Aunt Annie, who married Dave Murray, and they spent a number of years in Japan as missionaries. They later retired in Santa Monica.

In 1878 the family moved to Ottumwa, Iowa, where my grandfather built the first and for many years the only major John Morrell & Co. meat packing plant. I don't know what Eliza Thompson died of, a year after arriving in Ottumwa, but five years later my grandfather married my grandmother, who was also named Eliza, fortunately--Eliza McClelland. Now, she was Scotch-Irish. She had been born in northern Ireland, County Monaghan, in 1853. It was not a wealthy family at all, a rather poor family. She was the one who persuaded her family to come to the United States when she was a girl of about twelve. She persuaded them that they had no future in Ireland and that America was the country to live in.

So they came on a sailing boat in 1864, which seems remarkable in that period, when there were steamships, but apparently there were still emigrants who came by sail, particularly, probably, from poor countries like Ireland. She spent the rest of her youth in Washington, Pennsylvania, where there were family members.

As to how she came to Ottumwa, I don't know. I don't think she ever went to college, but she had training as a teacher, presumably at the young ladies seminary in Washington. In any event she came to Ottumwa in 1883, where she was principal of the Adams School, near the center of the town. I don't know how my grandfather met her, probably at church. They were both firm

Presbyterians. They were married in 1885, six years after the death of Eliza Thompson.

The first child of that union was my father's older sister, Ellen, who married Harold Bell, who lived in Maine. Then my father was born in 1887, followed by his sister, Edith, who was born eighteen months later. Then there was a brother, Morrell, and a second brother, Robert, who died in childhood, at the age of fourteen or so, probably of rheumatic fever. And a sister, Florence, who was younger also.

My grandfather Foster--as I say, I never knew him, but my father has talked about him, and although my father, I'm sure, never read Max Weber on the Protestant Ethic [*The Protestant Ethic and the Spirit of Capitalism*], that's the way my father described his father. My grandfather believed he was doing the Lord's work, and that his success in business indicated that he was one of the elect.

Riess: That's so interesting, the marriage of one of the daughters to a Hormel, the dynastic thing.

Foster: The meat packing industry was very different in those days from what it is today. They were all family-founded and run. In my youth they were all still controlled by the families. The principals of the various firms knew each other and liked each other. They got along well. It later became a very different business.

Riess: In fact it's a business that has to do with blood and guts.

Foster: Slaughtering animals is not a pretty sight.

Riess: But in your family it was viewed as an acceptable occupation.

Foster: Well, it was just viewed as an inevitable destiny for all sons. Just the way some people became doctors, following their parents, others became meat packers. My father would have much preferred to have been a university professor. He would have been much happier doing it, but he had a strong sense of obligation to the family.

In any event, I'll give you an example of my grandfather's character. He came to Ottumwa in 1878. I don't know what he did between 1871 in Chicago, and 1878, but he built the first packing house in Ottumwa then. I remember hearing my father tell about how he would design buildings. He'd go out with his chief carpenter, and take a stick, and on the ground, in the mud or the dirt, trace out the outline of the buildings, and say, "Begin here

and go up for six stories." The buildings were mostly built of wood.

Well, about 1892 he began building a big family residence on Fifth Street. It was a whopper of a house. I remember before it was cut down, it had twelve fireplaces in addition to central heating. God knows how many rooms. It had a conservatory, and out behind, on the other side of the alley, there was a barn for horses and carriages. And one summer evening in 1893 he was out in his buggy with my grandmother, taking an evening spin out west of town, and he looked east and he saw the sky all lighted up. And he heard the whistle of the packing house blowing, and he knew what was happening, what was burning up. He parked my grandmother with a farm family and ran the horse until it practically killed it to get to the plant.

Well, this was when he was halfway done with his new house. And I didn't know this until shortly before my father died thirty years ago--I had heard the whole story all my life, but this bit I never knew--he stopped building his own house for a year. He was afraid the Lord was telling him he was getting too big for his britches, so it took him a year to get up his courage to build, to finish the house. In addition to which, all the builders in the city were probably needed to rebuild the plant, anyway.

Riess: Is there something particularly incendiary about meat packing plants?

Foster: There's a lot of grease, and the wood timbers became soaked with grease. Yes, they will burn easily, particularly if they're wood.

Riess: Why didn't he stay in Chicago?

Foster: I think he felt that he was closer to the supplies in a small town like Ottumwa.

Riess: What kind of a town was Ottumwa? Who else was there?

Foster: It's seventy-five miles west of Burlington on the route of the California Zephyr, the Burlington Railway, 280 miles west of Chicago. It was a town that was established about 1848, a county seat, of which there are ninety-nine in Iowa, as I recall. It had a maximum population when I was growing up of about 35,000. I think it's down to 25,000 now and probably was about 20,000 in 1900. It was a small midwestern town, nothing exceptional about it at all.

Riess: Maybe it was originally founded by a religious group?

Foster: No, not by sectarian settlers, such as the Amana Colony, seventy-five miles to the northeast--but I really don't know how it was founded. That was the corn country on which pork was fattened. Beef was slaughtered, too, when I was there, but it was built as a pork packing plant, and that was the market in Britain, for smoked pork.

Father, George M. Foster

Foster: With respect to higher education, my grandfather thought universities were a waste of time for young men who ought to be learning the business from the ground up. He was more tolerant of his daughters, and agreed to let them go to college. And all of them graduated. My father's two older half-brothers [W.H.T. and T. Henry] had had a year or two at Parson's College, which is in the small town of Fairfield, Iowa, twenty-five miles east of Ottumwa. When my father went to college he went to Parson's and had a year there. Then, because he was always interested in engineering, he persuaded his father to send him to the University of Pennsylvania, where he had two years of engineering training.

He learned a great deal in that time. He was a natural for engineering. It's a great tragedy he wasn't allowed to go on. Today, of course, he would have rebelled and said "I'm going to finish, like it or not." But his father did allow him to spend a year with General Electric in Schenectady, where he learned a great deal more about engineering. So when he came back to Ottumwa in about 1909, he was just what the company needed. He was very well equipped to bring it into the modern period.

At that time, they decided they needed a second plant, which was to be in Sioux Falls. The science of agronomy was developing --they were developing means of raising corn further north every year, so the corn supply made possible raising hogs further north. So Sioux Falls turned out to be the logical place for a plant. It's still running today, I think a very good plant.²

My father was sent up there in 1909 or 1910 to plan the plant, which he did very well. I remember it well. He married my

²Several days ago I sat beside Margaret Berdahl [wife of the Chancellor of the University of California at Berkeley] and she told me her father had been general superintendent of the plant before his untimely death about 1950. She further told me her family's great fear was that they would have to move to Ottumwa! [note added by G.F.]

mother--they had been childhood sweethearts from high school on-- in 1912, the 12th of October, 1912. They remained in Sioux Falls until the fall of 1922, when my father and his brothers decided it was time for him to come back to Ottumwa to work at the headquarters. Those were the happiest ten years of their lives. They were away from their families, and they had a tabula rasa to make friends.

Riess: "Those were the happiest years of their lives." That's the way they would talk about it to you?

Foster: Yes. They were happy people all of their lives, but they remembered that as particularly pleasant.

Riess: This differs from the tradition of one son taking on the family business and another being permitted to go into the professions, law or medicine or something.

Foster: Well, it was just the British way--I don't know why it was, but it was assumed that everyone would go in the business. It was assumed that my brothers and I and all my cousins would also become meat packers. The oldest brother, William Heber Thompson, Uncle Will, was the manager of the Sioux Falls plant from the time it was built. He never wanted to be president, he knew he didn't have the ability. So the next brother, T. Henry, who was a very intelligent man, not widely liked by many people, kind of a cynical person--he was a good businessman, and he ran the company well, I think.

Riess: What's the "T"?

Foster: I think his name was Thomas. I'm Jr. My father was George McClelland Foster. McClelland was his mother's maiden name.

Church, Home, Politics, Wealth

Riess: The church. Your grandfather--would you call him deeply religious or was it more pragmatic?

Foster: I think he was pretty deeply religious. He believed in the Lord. He had no doubt about the hereafter, and all of that. That was the Bible Belt, after all. He was raised in the Church of England, but when he came to Ottumwa and found there was no Episcopal church he did the next best and became a Presbyterian. He wrote to his father notifying him of his change in affiliation, and the latter replied that he had no doubt that his Presbyterian

credentials would admit him to Heaven, but that he might find it necessary to sit in one of the rear pews!

He was a strong supporter of the church. He was a teetotaler most of his life, after his early youth. He supported Billy Sunday, I remember.

I was reading Kenneth Stampp's account, which I found very interesting--in spite of the difference in some of the aspects, very similar to mine, religiously.³ My mother had been raised as a Methodist, but she accepted Presbyterianism easily, so there was no conflict with my father and his family--we all went to the same church. It was fundamental Presbyterianism. My family was not quite as strict as that of the older brothers. I remember I felt so sorry for my cousins because they couldn't even read the Sunday paper on Sunday, the comics, they had to wait until Monday. In my family, that was allowed, we could read the comics, but that was about all. We couldn't go to a movie on Sunday. We couldn't play games other than--Bible anagrams was permitted, with my grandmother, who was very deaf by then.

Riess: That was a big family unit all in one town.

Foster: We were spread over two towns. Two of the brothers lived in Sioux Falls, and the other two in Ottumwa, and it was a long trip in those days. To get there in my youth we would take the train from Ottumwa to Des Moines, get on the Milwaukee sleeper, which went to Sioux City, Iowa, and then change and get on the train that went on up to Sioux Falls. So we were on three different trains for four hundred miles, for almost twenty-four hours. When we began driving it in the early days, before there were paved roads, it was a two-day trip. You were lucky if you did two hundred miles a day. It's hard to believe.

Riess: Did Sioux Falls feel more western?

Foster: Yes. It was a fresh, new town, and Ottumwa was an old town--it seemed old. I don't know when Sioux Falls was established, probably about a generation after Ottumwa, but it was the big city of the local area, and it's a very progressive town today, of a hundred thousand or so. I have not been there in many years, but credit card companies have many of their operations there, and the packing plant continues to be a major employer. Sioux Falls was a dynamic town, and it still is. Ottumwa was never a dynamic town,

³ Kenneth M. Stampp, *Historian of Slavery, the Civil War, and Reconstruction*, University of California, Berkeley, 1946-1983, Regional Oral History Office, The Bancroft Library, UC Berkeley, 1998.

in spite of the packing house being there. It was too bad for the city [Ottumwa] that there was only one major employer. I think there were three thousand employees in the plant at its maximum size, and that's just too many in a town of twenty-five or thirty thousand.

Riess: Other than the religious heritage, was there a value system that was a legacy from your grandfather?

Foster: Well, you worked hard. The fact that your family had enough money to live without working was no excuse for not working. We assumed we worked. When I say "work," I interpret that broadly. I never delivered newspapers, and I never waited tables in college, but I studied hard and did my best to accomplish something.

As I say, I was much impressed by the ways in which my background was like Ken Stamp's. He was much interested in scouting and was an Eagle scout. I was much interested in scouting and was an Eagle scout. And other ways. More recently, he crossed on the *Queen Mary* in the late summer of 1961, and we crossed on the *Queen Mary* in the late summer of 1961, which is kind of curious. He must have been on the next trip.

Riess: How about the concept of noblesse oblige?

Foster: My father always said, "Remember, you can't take it with you. You can't take your money with you." He said, "It's loaned to you, so to speak, and you want to make sure it's well spent." He was very generous to charities, local charities particularly, the YMCA, the YWCA, other churches, the Community Chest.

Riess: What was your own home like?

Foster: The home I was born in in Sioux Falls was a one-floor cottage. I don't remember it, obviously. It was a very simple cottage. My parents didn't have much money then. I think I have a memory of sitting in a high-chair in the second home in which we lived, with a gas light burning over my head. I just have a recollection of a flickering light. I must have been about two at the time. The last house we lived in in Sioux Falls, on Prairie Avenue, was an ordinary frame house my parents had bought. It had probably four bedrooms and a living room. Nothing out of the ordinary.

In 1922, when we came back to Ottumwa, we lived with my grandmother and grandfather Slutz, my mother's parents, for eighteen months or so, while our house was being built. That was a nice house, just a block and a half from my grandfather Slutz's house. The house my folks built was catty-corner from my grandfather Foster's house. It was designed by an architect from

Des Moines named George Kraetsch--I don't know how my parents found out about him.

But just about this time, in 1921, my father's next oldest sister, Ellen, had lost her husband, Harold Bell, to cancer, and she had come from Portland, Maine, back to Ottumwa to live. She, with her boys, moved into the old home where my grandmother was still living. That house was cut down from its size to a house that's still a big house but no longer as big as it was.

Riess: Cut down?

Foster: I think the third floor was turned into an attic, and the conservatory was moved away. It was still a large house but not nearly as large as it had been. It had had twelve fireplaces, so it must have had at least eight or nine bedrooms. There were always British cousins coming and staying. I remember a set of pottery from England called Liverpool Lustre, which is brown on white. My mother inherited a good deal of it, and then Mickie and I inherited it from them, and we gave it to our granddaughter--our daughter-in-law, Angela, who's British. That had service for thirty-six people and included twelve platters, so that's--you just can't believe that kind of a life!

The house my parents built, at 401 North Market St., was brick veneer. That is, it looked like brick on the outside, but it was basically frame, with bricks on the outside. British Tudor. Phony timber frame. There was a steep slate roof, chimney pots. It was a type of architecture that was being widely followed--this was 1924 when we moved in--in that period, in the whole Midwest. It was one of the nicest houses in the city. For some reason, I remember my father saying that the contract was \$40,000, which was a lot of money in 1924 for a house.

It had a large living room, and a study, both with fireplaces. A big entry hall and a smaller entry hall where overshoes were left and coats were hung up. As you came in the front door, you walked by the living room, directly into the study. If you turned right before you got to the study, you were in the living room. The stairs to the second floor, which were very steep, went up from the left of this front hall.

The dining room, which had a nice view to the south, over the town, was backed up by what was initially a playroom for my brother, Bob, [Robert Morrell, born 1916] and me.

Riess: He was younger?

Foster: He was younger, yes. He died in 1986. My brother Gene [Eugene Moore, born 1921], who followed, is still living with his wife, Joan, in San Diego, and my sister, Janet [Mrs. Thorndike Saville, Jr., born 1927], is in Washington with her husband.

Then there was a small kitchen and a good-sized pantry. The driveway went up the northeast side, off Market Street, up behind the house, where there was a two-car garage and a "Y"--to turn the car around. It was not a big lot, and it was much too big a house for the lot.

My father felt he should be near his sister, Ellen, to help her since she lost her husband and was on her own. And that was the neighborhood in which he had grown up, and there were other family friends--Greenleaf Merrill (Sr.), his best friend on earth, lived a block away. The folks had debated building out on the edge of town, but decided on building in the middle of town, which was easier for us because we were only two blocks up the hill from the main part of town, a block and a half from my grandmother Slutz, my mother's parents, and nearer to schools than we would have been otherwise.

It was a good neighborhood to grow up in because all the families had children. Within a block of 401 North Market St. there were at least twenty kids within two or three years of my age.

Riess: And heterogeneous.

Foster: If you mean racially heterogeneous, it was nearly totally white. Economically, there was more variation. Phil Pratt, who lived across the street, his father worked in the post office. He, interestingly, was one of the few that went on to graduate school. He's the only one that's still alive, as far as I know. I'm in touch with him. I was interested in comparing my background with Ken Stamp, because all of us in that neighborhood assumed we'd go to college. Phil assumed he'd go to college, and I think we all did. They were not wealthy families, they were not even well-to-do, many of them, but they had the means to send their children to school, to college, and all of us assumed we would go to college, and all of us did.

Riess: Did you use a town library or was your home library sufficient?

Foster: I pretty much used the home library. That was probably a mistake because I would have been exposed to more, but I was exposed to a lot of good things. We had a complete *Encyclopedia Britannica*.

Riess: Which you read through?

Foster: No, I did not, no. I checked things in it. We had a complete set of Mark Twain, which I did pretty much read through and enjoyed immensely. We had the complete works of O. Henry, which I didn't read. My father was interested in the history of the West. I don't think he ever read [Frederick Jackson] Turner, but he had a good many books on [John Wesley] Powell and things like that, and I read those. We had some contemporary literature, novels, in our home library, but not a great many.

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Foster: I read the standard boys' books, the Tom Swift books--"and his flying machine, his submarine," everything. Jules Verne, I read all of Jules Verne. Let's see what else? *Youth's Companion* was the magazine that was for boys. We laugh, because I was the first one to subscribe to it and it came to the house for about twenty years until my sister, who is thirteen years younger than I am, decided she was no longer interested. We thought that they must think here was a very retarded child, after twenty years still reading the same magazine.

Riess: How about European writers? Tolstoy?

Foster: My family didn't know about that, and I was not exposed to it. Of the great novels, I can't remember--I have a miserable memory for my childhood, really. But I read a great deal. There was no radio or TV in those days, and reading was the only thing to do. I was sufficiently good at school, so I rarely had homework. So I read every evening until bedtime, and weekends, too, when I was not out. My parents had a good eye, my mother particularly, for the books that people my age would enjoy. We were early readers of the Dr. Doolittle books, for example. *The Wind in the Willows* and things like that.

Riess: Do you remember when you were younger your mother reading to you?

Foster: No, she didn't read to me much. The reading we got was from Hurlbut's *Book of the Bible*, Bible stories. My father and my mother both would read sometimes from that, and that was where I learned most of what I know about the Bible until I went to Mexico and began, through the fiesta system, learning about the saints and everything like that.

Riess: But you did go to Sunday school.

Foster: Yes, indeed. I didn't mind that, but I hated church. Every Sunday I'd get a terrible headache. I just hated Sunday coming around because I knew I'd get a sick headache and spend the afternoon in bed with a wet washcloth on my forehead.

Riess: Literally? You weren't faking it.

Foster: No, I wasn't faking it. It just came.

Riess: What was so bad?

Foster: Well, I've often wondered. I just found--here again, I sympathize with Ken. I found the sermons so boring, and I just would get nervous sitting there. [pauses] If it had been only the music, which--we had good music in our church--I would have liked it, I think. I don't know, it just seemed like such a waste of time.

But I was a firm believer--I would say even slower than Kenneth in giving up my religious beliefs. I remember, though, asking my mother how did one know how this or that was true. She, in exasperation, finally said, "Well, there are just some things you must take on faith. You can't know about them." Well, I couldn't take it on faith, although I had internalized it and accepted it until I was in college.

I remember a moral problem of great magnitude that faced us when "The Ten Commandments," the movie, the first movie, came to town. It was shown only in the evening and on Sunday afternoon, and the question was, would we be kept up after our bedtime to see this great religious work or would the Sabbath be violated? And the Sabbath was not violated. [chuckling]

It's unbelievable. Our upbringing was strict, you might say, but we had no doubt that we were loved and wanted. So that was more than compensation.

Riess: It probably wasn't very different from your peers in town?

Foster: I don't think they were quite as religiously rigid as we were in my family. Many of them didn't mind playing golf on Sunday, and would not hesitate to go to a movie on Sunday.

Riess: I was wondering--you had a large house, but you also had a large family--did you need to try to find privacy for yourself. Was privacy an issue?

Foster: Not until I got in high school. My brother Bob and I shared a bedroom when we moved into the house. He was born in 1916, so I must have been eleven when we moved in, and he was eight. And until I was in high school we shared a bedroom. And then when I was in high school I moved to the back bedroom, which was a small bedroom which had an adjoining bath shared with the guest room. So I had my privacy there, but I really didn't use the room very much, except for sleeping.

It was a big house. The living room was a big room, rarely used unless there was a party of some kind. The study, the library was the room that was home. It had my father's desk, and there was a big fireplace and a wood lift and books. He had his easy chair, which I would use when he was not sitting in it.

Riess: [chuckling] Did that feel good?

Foster: It was a comfortable chair, I remember.

Riess: It was a seat of power?

Foster: Well, I didn't think of it in those terms.

Riess: But I guess I didn't mean privacy quite like that. I meant more, were you allowed to develop your own set of thoughts?

Foster: Yes and no, I think is the answer--I know your question. I was interested in my father all my life. In his profession he was a really interesting thinker. Politically, he was conservative. In his later years, particularly after the New Deal came into power, and he was struggling to keep a packing plant going, he was afraid that in college we would be seduced away from the straight and narrow path, which was Republicanism.

I must say that I have thought about the way society was structured in Ottumwa. There was only a very small Negro population, maybe two percent or so. It was assumed that the Republicans, who were largely Presbyterians and Church of England, Episcopalians, with a good dose of Methodists, were the upper class, and this was almost a genetic superiority. The next middle class were the Democrats, who tended to be Catholics and Baptists, and it was assumed that they were somewhat genetically inferior but doing the best they could. And then the blacks were the hewers of wood, the carriers of water--honest, good people but genetically inferior.

Riess: Marriage between a Democrat and a Republican would be intermarriage?

Foster: I remember it was looked upon askance when one of the Mahons, who were wholesale grocers in town, married a woman who was a Democrat during the New Deal and was appointed postmistress, the first time a Democrat had ever had the job in this Republican town. It was kind of funny.

Riess: Is this a retrospective kind of view of town or were you pretty aware of it when you were there?

Foster: I was totally unaware of it then. This is my retrospective analysis. I accepted everything about it. I just assumed this was the way the world was.

When I compare my politics and my growth with Ken Stamp's, I feel I was not very original. I never went through any major crises. My religion just began slipping away, I think. In my freshman year at Harvard I broke down and went to a movie on Sunday, and I was astonished that the Lord didn't strike me down. It made me doubt that he was even there. And that was really the beginning of my slipping away from the church.

My geology course with Kirtley Mather at Harvard was really a marvelous experience. It started me thinking about life in general, and I came to see that there was more to life than I had known. Certainly my father believed in evolution. My mother did too. There was none of this nonsense of the religious right of today. But my mother knew I didn't like church, and when I got to college I didn't go to church much, and I've never gone since. For a long time she kept saying, "Well, I know, Sonny, you'll come back to the church someday."

She pretty much faded away from the church herself, as matters turned out. As she became older--she lived to be ninety-five, she was a remarkable person--she began to seize upon her deafness as an excuse for not going because she couldn't hear the sermon. [chuckling] And I don't think she really believed in a personal hereafter at the time she died. Considering her background, she was remarkable in her adaptability. But she always believed there was some greater force, some spiritual force in the universe.

Riess: You mentioned the New Deal. How much of an effect did the Depression have on you and the town and so on?

Foster: Well, in our family it had no effect at all. We had all the money we needed all the time. The question about spending money in our family was moral, not financial. If you wanted to do something, the question was not can we afford it but is it morally best for the child. An example I remember happened when I was about sixteen, about halfway through high school. My cousin, Henry Huntington, who was known as Tertius, the third Henry Huntington, lived in Scarsdale with his mother, my father's next younger sister, Edith--his father was a member of the Huntington clan of Boston. Ellsworth Huntington, the geographer at Yale, was one of his brothers. He [my uncle] was kind of the black sheep--well, not the black sheep, but he was not a very successful man. [laughs] Actually he was kind of a screwball, and in his sixties or seventies he became a nudist.

My cousin and his mother invited me to come down at Christmas to spend the holiday. That would have been about 1929. I had been to New York, but never alone, and I was very anxious to go. I remember we had a family debate for several days as to whether I should be allowed to go. Finally, I was allowed to go. That's the kind of situation it was.

Riess: This was from Ottumwa?

Foster: Yes. It was two nights and a day on the train, each way, a long, long trip.

Riess: I guess your parents had to figure that they had raised you up as they would have you be.

Foster: Yes. My father, well into my adult life, would send me clippings from newspapers with the implication that I'd be a better person if I'd read this. As a consequence, I find it very difficult to take anyone's recommendation on reading anything. Mickie [wife] tells me, "This will be good for you if you read it," but I say, "That's the worst recommendation you can give me."

Riess: You don't tear up the newspaper with something for one person, something for another that they're sure to love to read?

Foster: Very rarely, and I never suggest that it will make them better persons.

I don't want to give an erroneous impression of my father. I admired him greatly, and I'm like him in many, many ways. He wanted me to be an engineer, and I--well, I'll go back a little before that. I think that men probably express their relationship with their fathers through politics. My grandfather Foster, although he was the principal citizen of the town, did not take out citizenship in this country until twenty-five years after he came to Ottumwa. When the "old queen" [Queen Victoria] died, then he was able to become an American citizen. He registered as a Democrat, which my father's older brothers were also.

My father, when he came of age, registered as a Republican. He said frankly he was rebelling against his father. That was the only way he could rebel. He wasn't able to kick the traces and say, "Damn it, I'm going to be a university professor."

I have maintained my Republican registration. But I haven't voted Republican in thirty years, I don't think. It's kind of an interesting thing.

Riess: Were there political discussions in your family? Was politics a good topic at the dinner table?

Foster: Well, there was not enough difference of opinion to make it very exciting. It was assumed that the Republican candidate was the logical candidate, and the Republican policies were the logical policies. Until I met Mickie, whose father was a firm Democrat, I didn't realize how different politics could be, and the way people looked at life.

Riess: You're describing, in some sense, the ideal situation in which to flourish. The question is, when does a person start to ask questions?

Foster: It was comforting to a point, but stifling in the long run.

Riess: So having a headache on Sunday--

Foster: --was a form of rebellion.

Mother, Mary Moore Slutz

Riess: Your father hoped you would be an engineer. Do you think your mother felt the same way?

Foster: Oh, yes.

I must talk about her family. That's equally interesting. I knew both of her parents very well. We lived with them for eighteen months in their house when we came back from Sioux Falls to Ottumwa. I was particularly attached to my grandmother. As a small child, I remember, we'd go to Ottumwa every summer for several weeks, and on hot nights she would take me up to the roof of the house, which had a flat area about ten feet or twelve feet square, safely enclosed by a balustrade, and lay out a thin mattress and a blanket, and we'd lie there. [pauses, emotionally overcome]

She was a remarkable person. She was born on a farm near Ottumwa in 1855, and my mother was born on the same farm in 1887. My mother's two older sisters were also born on the farm. I don't know where she met my grandfather, Benjamin Franklin Slutz, who had come out from Ohio--he had been born in Ohio. I don't know how he came to Ottumwa. But they had met and married when they were young, and they had four children. The eldest was Winona, who was born about 1883, I suppose, or '84. Grace was the second,

who was born about 1885, and my mother 1887, and Jim, the baby of the family, was born in 1890.

They had a strong sense of the value of education, particularly my grandmother, who went to college for a year or two herself, at a little college called Iowa Wesleyan in Mt. Pleasant. She'd had a couple of years of college there. One of her classmates was a well-known Harvard economist named Thomas Nixon Carver, whom she kept in touch with all her life. He was my advisor the year I was at Harvard.

I remember my grandmother saying that her father, when he came to Iowa, brought a chest of books, two of which were--first was the Bible, and the second was Noah Webster's dictionary. I believe he was German in ancestry, and had the German interest in education. My grandfather, I'm sure, had no college, but my mother and all three of her siblings were graduates of college. The oldest three went to Grinnell College. No, Grace, the second one, because of ill health, was sent out to Boulder, where she graduated from the University of Colorado. The other three graduated from Grinnell.

My father would have loved to have graduated from college, too, so there was no question but that all of us would go to college. And as I say, that was expected of everyone, all of our friends in the city. There was a college preparatory program in high school, and there was a commercial program. They had no hesitation to track kids then, which they wouldn't do today, of course.

Riess: But what about looking at the stars with your grandmother? Was that a moment of asking what it's all about?

Foster: Well, I think I was more excited about being up above, on the roof of the house, looking down, not knowing whether I should be frightened or not. What I remember is it was just wonderful to lie up there with her. I can't remember--I was five or six or seven then--I can't really remember what we talked about.

My mother said often, kind of irritatedly, "George, you liked your grandmother more than you like me." Once she asked me, "Well, why don't you go and live with her?" I thought a moment and said, "No, she's not new enough." [chuckling] I guess I was scheming from the beginning--not scheming, but sizing things up.

My mother and my father were an interesting pair. My mother, from her mother, developed a tremendous sense of rapport with everyone. She often quoted her mother, saying, "When you meet people, be prepared to go more than half way." I've often

thought how true that is. My mother, in spite of her feelings about the superiority of Republicans and Presbyterians, had a sense of the feelings of everyone.

One of the things I'm proudest of--[emotional] this is a little story--about 1936 Marian Anderson came to town and sang in the high school auditorium. And as was the custom, there was to be a reception for her afterward, and my mother was the one who gave the reception. She said later, "Before the reception, I began thinking, here's this wonderful singer, and nobody of her race was there. We're all white." She knew the names of the ministers of the African Baptist and the African Methodist church and she called them up and invited them and their wives to come to the reception, and they both came. This was certainly the first time there was racial integration in that conservative little town.

About a year later my parents were in New York, staying at the Waldorf, and they got in the elevator and Marian Anderson got in the elevator, and she recognized them.

Riess: Your mother certainly had the right instincts.

Foster: My father was very reserved. He was viewed as a very--how would I say? He found it difficult to warm up to people, to reveal his feelings. My mother was very, very good for him because she was the one who could make small talk and ease situations. She was a much-loved person. [emotional]

Riess: Nice memories. And you're lucky that she lived so long, too.

School and Community and Individuality

Riess: I had thought that more of your life had been spent in Sioux Falls, and I was interested in whether you had encountered Indian communities there, or other ethnic groups.

Foster: No, no.

Riess: Did you have any curiosity about other ethnic groups, or maybe there weren't groups to be curious about?

Foster: No, no. That all came later, and was a byproduct of reading, I think, rather than experience.

Riess: And your contact with the African Americans in Ottumwa?

Foster: Well, we had black servants in the house, and in our scout group there was one black kid, George Jackson. The view toward the blacks was curious. They weren't allowed to go into restaurants, and in theaters they sat in the balcony, but there was no hesitation in taking them into the Boy Scouts. I remember this black kid at camp, who was a couple of years younger than I, was a patrol leader. He had all white kids under him, and no one thought it was odd.

In retrospect, I've never figured out--his father, also George Jackson, was manager of the country club. With a different opportunity, he would have gone a long way in business, I think. But our contact with blacks was essentially as servants.

Riess: Were your high school teachers important mentors?

Foster: Some of them were very important. I had a marvelous grammar teacher named Miss Naines. She was a martinet. I always felt she didn't like me, but I learned a great deal from her. We parsed sentences. She always curtly addressed me as "Foster." All the other teachers called me, as they did all the students, by their first names. But I learned a great deal from her.

And history. I loved modern European history. We had two sisters named Smith. One was an English teacher, and one was a history teacher. Edna and Olga. Olga was the historian, and she really made history come alive for me. I've enjoyed modern European history ever since. I have a good memory for dates. When I think of history, I look back and I just see the years preceding, going back. When I think of independence, 1776, I see the figures. I see the Battle of Waterloo, 1815, or the Seven Years War 1756 to '63.

I was astonished to read that in a survey recently most of the kids couldn't even tell the century in which the Civil War had been fought. I found that just unbelievable. I sometimes think I remember more of the dates than I do of what went with them.

Riess: Can you talk about her method of teaching?

Foster: Well, she talked a good deal. I don't recall what the method was, but she did something that caught--well, the two sisters were top-rated by everyone. As I say, she really excited me.

Riess: Did she give you any ideas of what you could be?

Foster: No, I was not open to any ideas as to what I could be. I knew what I was going to be. I was going to be a meat packer and an engineer.

Riess: Engineering training, applied to the meat packing business.

Foster: I'd be an engineer, and I'd come back and follow my father into the plant and maybe someday be president, the way he was. I didn't question that. I'm amazed at how unquestioning I was. If I had been more open, I would have become a geologist. Kirtley Mather's geology course at Harvard really, as I said, turned me on, and I was wild about it.

The class work at Harvard was a great deal harder than any study I had ever done anyplace before, but I loved the weekend geology field trips. I remember I had the second-highest grade in the geology class in the final examination of the whole year. The grades were posted, that's how I knew.

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Riess: Thinking back, can you remember a time when you felt your self as a separate individual?

Foster: I'm not aware of any specific time when I became aware of myself as a unique individual. I remember it was something I was grasping for for a long time. As a member of a large family, and identified in terms of a large family, I felt that I was not exactly hemmed in, but I wanted to be on my own, to feel that I was an individual on my own. I remember two instances in which I wasn't, or that I suspected I wasn't.

The first was once when I was on a train coming from Denver to Ottumwa, and someone in the observation car came up to me and asked, "Are you a member of the Foster family of Ottumwa?" I had to confess that I was.

The second time was in the doctor's office in Chicago, Dr. Ernest Irons. My name was called, and I went in to see the doctor, and when I came out, the nurse said, "You're from Evanston, aren't you?" and I said, "No, I'm from Ottumwa, Iowa." She said, "That's interesting because there was a woman here who was waiting to see another doctor who, when your name was called, asked, 'Is Mr. Foster from Ottumwa?' and I said, 'No, he's from Evanston.' I thought you were from Evanston." I said, "No, I'm going to school there, but I'm from Ottumwa, Iowa."

I felt that I must look a lot like--there must be a strong family resemblance in both cases.

It was not until I came out to Berkeley as a graduate student in 1935, where no one knew me, that I really felt that I

was on my own. It was a wonderful feeling of liberation. I'll always remember it.

Riess: Did your brothers have a similar response?

Foster: Yes, they felt the same way. My brother, Bob, was in the packing house for a number of years, but he was very handicapped because he had suffered from mastoiditis. He had two operations for mastoiditis, double mastoid operations, and it left him very deaf all his life.

I don't know where he went to college. I think he started out at Northwestern, but he didn't graduate. He married, when he was only twenty, Dorothy Merrill, the daughter of my father's and mother's closest friends, Greenleaf and Mary Merrill. He moved to Sioux Falls shortly after his marriage, and he had somewhat the same experience as my parents. He was there for ten years or so and he loved it, and then for some reason he was brought back to the Ottumwa plant, and he felt a little closed in there. Finally, he threw in the towel and moved out to San Diego about 1957.

My next brother, Gene, who is eight years younger than I am, stuck it out in Ottumwa for a few more years; then he also threw in the towel and moved out to San Diego, where he now lives with his wife. So I know they had the same experience, but they were not as lucky as I was in that I found an exciting occupation early enough in life to do something about it.

Travels

Riess: Did your family travel during the summers? What I'm getting at with this question is whether there was in any way a family curiosity about other places and other cultures and so on.

Foster: Yes, I think there was. My grandfather Foster--I remember one of the things that most amazed me about him was that he crossed the Atlantic fifty-three times. When you think the last time was about 1912, that was a lot of trips. It must have been twenty-six and a half, I figure. Some years he went back twice to Europe.

I remember my father telling about a marvelous trip that he made with his father and mother and several siblings in 1900. I know the date because he told about going to the Oberammergau passion play. He said he was thirteen at the time. I remember him telling how he was riding on the roof of the stage coach along the Rhine, having had some wine for lunch, and feeling quite

tipsy. Quite an unusual experience for him. He described his father as having invited the British cousins, of whom there were a great many, to come along on the land part of the trip. He said he had a whole grip full of tickets. He remembers that very clearly.

My first real foreign experience was in Porto Rico in 1927, when I was thirteen.⁴ My brother, Bob, and I went to spend six weeks with my mother's sister, Grace, who had married a tobacco planter from North Carolina, Al Walker. He later got into sugar and was a sugar planter in Porto Rico. They had an elaborate place in a town called Humacao. My brother, Bob, and I met my cousin Marian, who's an age between Bob and me, the older of the two children of Grace, in New York. We stayed at the Vanderbilt Hotel, where Al Walker had an apartment. We three kids, with a secretary named Jane Smith, got on the old "San Lorenzo" of the New York and Porto Rico Steamship Company.

We were on board four days before we got to San Juan. I mention this because I've always been fascinated by boats. I wish I could remember birds as well as I can remember boats. As long as I can remember, I've had this fascination with boats, and I still have it.

This was a very interesting time because Porto Rico was still very strongly Spanish in influence. It was not overcrowded the way it is now. My Aunt Grace, my mother's sister, Grace, had gone to Porto Rico as a maiden lady, to teach school. After the Spanish-American War, a good many American young women went either to the Philippines or to Porto Rico to teach school, and that's where she had met Al Walker. She lived there from the time of her marriage, which was, oh, fifteen years prior to my going there.

We had a wonderful time in Porto Rico, Bob and I. We had an extra week because I fell off a horse and broke my arm. There was no telephone to the States in Porto Rico in those days. The mail left once a week on the mail boat. Bob was just finishing his letter when I was brought in, having falling off the horse, and he signed his letter, saying, "George has just now fallen off a horse. I don't know if his arm is badly broken or not. Your loving son, Bob." [laughter] So we waited until five days or so and sent a cable that I had broken my arm but I was coming along fine, and that I'd be a week later on the boat.

⁴I use the term Porto Rico because Puerto Rico did not become common spelling until many years later. The steamship company, for instance, was the New York and Porto Rico Steamship Co. G.F.

Riess: That was the first extended time away from your family?

Foster: No. In 1922, just before I left Sioux Falls for Ottumwa, I spent the summer with my father's next oldest sister, Ellen, and his next younger sister, Edith, at North Scituate [Massachusetts]. They went there every summer with their kids, and I was invited down, so I was there for six weeks or two months. An interesting thing psychologically was that my father came down to pick us up--he brought Bob down, too, to stay for a few days--and on the train going back, he said, "We're going to move to Ottumwa." I was heartbroken, and Bob was upset, too, and we both developed tremendous hay fever and asthma on that occasion, which bothered us for many years.

Riess: Do you think you had been sent away at that point because the family was in the throes of making a decision?

Foster: No, I don't think so. There was a lot of visiting back and forth because it was a close family, and still is quite close. It was just customary for the cousins to like each other.

Riess: Did your family take driving trips?

Foster: Yes. The earliest driving trip I can remember was when I was, I suppose, about eight. It was in the late summer. We were driving to Minnesota, which is cooler and prettier country, wooded country, the north part of the state.

I remember on one occasion [laughs]--I don't know why my folks didn't get rid of me then--I had stopped to go to the bathroom on the edge of the road, and I stepped on the tar, wet tar, and got my shoes all dirty, so they had to stop somewhere and buy me some new shoes! And when we got to this lodge where we were staying I walked right down off the highway, past the houses, right out to the pier, right off the end of the pier, which was fortunately only about up to my chest. I was not a very promising kid at that point. [laughter]

Actually there was an earlier trip, in 1917, when we still lived in Sioux Falls. I remember the car, it was a 1917 model Hudson sedan. It stood seven feet high, I remember my father saying. We drove up through Minnesota to Duluth and got on the boat, a boat called the "Oftarara", and went to Mackinac Island, where we spent two or three days at the Inn there, that wonderful old white wooden lodge. Then we took another boat called the "Manitou" down to Chicago, from where we drove back to Sioux Falls. It was a long, long drive in those days.

Then another trip--well, we drove into Minnesota several times. We liked to go to fishing camps.

But after Porto Rico, the next two summers Bob and I went to camp in Colorado, in Estes Park. We would go for eight weeks each summer. That was a wonderful experience.

Riess: What did you learn in those periods of time?

Foster: I learned how to become a good shot with a rifle, thanks to the National Rifle Association, which I'm ashamed of. They had a very good way of enlisting the support of kids. You had a series of bars depending on--you were allowed to work up to the tenth category. But in addition to that, I learned camping, packing. I had been a scout. I was a scout at the time, and I knew a lot about camping. That was one of the good things for my profession as an anthropologist. Going to Mexico, out in the wilds, I had no hesitation about going. I knew how to pack and what to take.

Those two summers at camp were just a marvelous, free experience. Good friends from other parts of the country.

Riess: A part of my interest in your travel is whether your parents expressed an interest in other cultures. Was there a way they would reflect on where you were that helped you come to realize that there are--. Did you have a developed sense of "us" and "them"?

Foster: Well, mostly we were "us," in that small town.

Riess: So what did you learn about "them" when you would travel?

Foster: One of my earliest experiences of "them" was when I went to Chicago Heights when I was about twelve to visit with my Aunt Winona, my mother's oldest sister, who had married a fellow from Alden, Iowa, named Walter Spencer. They lived in Chicago Heights. Walter was very interested--he loved Italian food, and I had never known there was such a thing as Italian food. I didn't know there was any food other than straight Midwestern food. I remember he took me to a restaurant with Italian food that opened my eyes.

He took me to a glass blowing factory. I can still see those central Europeans with their cheeks bulging out, that glob of molten glass on the end. That was a very exciting thing for me.

Riess: Yes, I want to know about that eye opening. I want to know about when you became a curious person. Were you a curious person when you were a high school lad, or were you doing what was put in front of you?

Foster: Well, I think I was fairly curious. I mentioned the fact that I asked my mother how we can be sure, and she said, "You have to take it on faith," and I couldn't take it on faith.

Riess: Were you interested in science, in the questions that are raised by biology and physics?

Foster: Well, I was interested in--I liked school. I didn't dislike school. I did well. I didn't do exceptional--I didn't work awfully hard, but I didn't have to work awfully hard to get fairly high grades. But I must have read a good deal of science. I knew a lot of facts. That was one of the reasons I became an anthropologist--and I can tell you how that happened, but maybe we ought to save that for next time.

Riess: I ask those questions because I am interested in when people question the meaning of life. I'm interested in whether that "what's it all about" question comes up for you, or if it appears to be explained by religion or whatever.

Foster: I was kind of a mixture, I think, in that a great deal I just assumed that's the way the world is, I didn't question it. On other levels, I wanted to know why. I must have read a good deal of natural history, but I can't think--. And my father read widely, and he talked to me a lot about engineering problems. I knew the difference between a reciprocating engine and a turbine engine, for example.

He would take me to the packing plant engine room often when I was a small kid. I remember the smell of ammonia from the ice machines, and it made my eyes sting, I could hardly go in. But I remember these marvelous fly wheels going around, and the pistons moving back and forth. He explained the purpose of the--I forget what it's called--something that controls the speed of the machine. There were wonderful devices. And I liked that, I was interested in it. But I never became interested in engineering as such.

A Year at Harvard

Riess: Tell me about the college decisions. First you went to Harvard for a year, you say?

Foster: Yes. My father--both my folks thought it would be better for me to get away from the Midwest to the East Coast, just because they recognized it was a different culture and it would be a valuable

experience. They were quite right. Well, a lot of us went east. Four of us went to Harvard that year from Ottumwa, and I think three more went to Swarthmore. If I had gone to Swarthmore, which was the other college I considered, it would have been four there, and three at Harvard.

In the spring of that year, my mother took me on a trip to Boston to visit Harvard. Then we went to New York and down to Philadelphia to look at Swarthmore. I don't know why I decided on Harvard. I guess I was ready for the big time, and I thought it would be good to get something more than a small college. I think I was right there, I think it was true. So there were four of us who went to Harvard. And there were a couple from Ottumwa already there, so we had quite a contingent.

But it was not my happiest year. I made a terrible mistake, and I don't know why my folks let me make it. I roomed with Greenleaf Merrill, from Ottumwa. We were best friends, and that was a tragic mistake because we cut ourselves off from the opportunity of tapping into new networks of friends. And because we were in a dormitory where all the other kids had come down from prep schools, they had their own friends, they weren't interested in us. I didn't even know the names of the kids on the other side of the entryway. Just unbelievable.

Harvard was hellishly hard but very exciting. I remember the history course, European history, was very good, and geology, as I say, turned me on. We had a very good English teacher. That's where I began reading more modern literature. I remember *To the Lighthouse*, Virginia Woolf, was my first experience. We read some old clinkers like *Return of the Native*. Mr. Gordon was an excellent teacher. I suppose he was a teaching assistant, though he seemed very advanced.

Then I had French, from Mr. Lincoln, who was a dull teacher and teaching, for me, a dull subject, which I knew I'd never learn. The other topic was mathematics: trigonometry and advanced calculus, which I did very poorly in. I never understood it. But in spite of that, I got pretty good grades for the year. I had two B's, two C's and a D, which at a time without grade inflation put me on the dean's list.

I wasn't taking engineering classes, but I remember I went to talk to the dean of the College of Engineering, and asked him about entering the college. He said, "I urge you to get your degree in liberal arts and then study engineering." It was good advice, but I wasn't ready to take it. I had culture shock, really, going to Harvard, out of a small town in Iowa. I didn't know that the cause was what was later identified as culture

shock, but I was very depressed. So when the opportunity came to transfer to Northwestern, which had quite a good school of engineering, I decided to go there.

Riess: Did you go home for Christmas vacation from Harvard?

Foster: Yes, and that was a long trip, two nights and a day each way on a train.

Riess: Were you in touch with home by telephone?

Foster: No. Long distance telephone service was hardly known at that time, and very expensive. Just a weekly letter. I found that a weekly letter was a good way to keep people at a distance.

Riess: What do you mean?

Foster: I mean I would write a letter, and that's all I'd have to do. My parents weren't dropping in on me all the time, I could control to a degree my contact with them. And I could maintain my separateness that way, in a way I wouldn't today with immediate contact through e-mail or telephone.

Riess: Did they write back?

Foster: Yes, they wrote. As long as my parents were alive, I wrote a weekly letter, and I'd get almost as many letters back from my folks.

Riess: Did they continue to be parental?

Foster: Well, I guess for a while.

Riess: That first year at Harvard, did they know how unhappy you were?

Foster: Oh, yes, they knew. I remember my father said, "We tried to teach you how to live. You're on your own now. We're sure you will not disappoint us." So they put trust in us. They didn't worry about us, I don't think. And we never violated that trust.

Riess: The culture shock. Did some of your compatriots come back from Harvard after that first year, too? Your roommate, for instance?

Foster: He's the only one that graduated. He never married, and died in his late forties, bedridden with Parkinson's disease for the last several years. Blake Garner, his cousin, came to Northwestern, along with me.

Riess: What was his name?

Foster: Blake Garner. He was a cousin of Greenleaf Merrill. And "Dutch" [Garner] McNett, who was also Greenleaf's cousin, stuck it out till his senior year when he gave it up to marry his girl, Jean Lowenberg, from Ottumwa, who was going to Wellesley. They lived happily for sixty years, until Dutch died about five years ago. Jean is still alive and peppy, judging by a letter I received from her earlier this year, from North Carolina, to which they had retired from Cambridge, Massachusetts, some years earlier.

Riess: What languages had you studied in high school?

Foster: Latin and French. I had a couple of years of French. But I had a block on languages. It never occurred to me that I'd ever use a language to communicate with people. When later I began thinking about it, I could see the reason. To hear languages spoken as the native language of a country--if I went east I'd have to go five or six thousand miles; if I went west I'd have to go eight thousand miles; if I went north over the North Pole it would be six thousand miles; south to Mexico was fifteen hundred miles and they were having a revolution there.

I use this example when I talk to Europeans who express amazement that Americans are such poor speakers of foreign languages. The idea of using a foreign language to communicate was just totally foreign, it never occurred to us. As I say, I had no natural talent for language. Subsequently I learned to speak fairly passable Spanish because I had to. I've learned to read French, I can read French pretty well, and I can speak a little of it. German I learned to read twice and forgot twice.

Riess: I grew up in a community where they spoke a German dialect.

Foster: There were no dialects at all in Ottumwa. We all spoke without accent. [laughing]

Riess: Even among the packing house workers?

Foster: I don't recall any ethnic dialects, ethnic populations, really.

Transfer to Northwestern

Riess: Was Northwestern the obvious choice if you were not going to be east?

Foster: I think it was, yes. I enjoyed being close enough to home so I could go home occasionally. On my birthday, the 9th of October of my sophomore year, my first year there, my mother gave me her old Essex car, and that buoyed my spirits. Having driven since I'd

been a freshman in high school, it was just terrible, my year at Harvard, not having a car. Nobody else had a car, but I missed it terribly. And at Northwestern, this old jalopy she had was a great stimulus. I'd go home every six weeks or so.

Sometimes I'd take the train. I've always enjoyed trains, transportation in any form. Mickie's always commented about how we in Ottumwa always knew the numbers of the trains: "So-and-so is coming in on Number Nine this afternoon," or "I'm taking Two into Chicago tonight," or "He's arriving on Three," or "going in on Six."

It was only two hundred and eighty miles, but it was eight hours on the train, and the train left about 11:30 at night on its way to Denver and got to Ottumwa at 7:30 in the morning. I'd put my bag under the berth. Then I'd walk the whole length of the train, up to where the steam engine was huffing and puffing. That was a marvelous sight, just very exciting. Then I'd go back and get in bed and be lulled to sleep.

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Riess: Did you live in a dormitory when you were at Northwestern?

Foster: No, I became a Phi Delt because Larry Spillman, who was one of my classmates in Ottumwa, in high school, who later went to West Point and graduated, had gone for a year there, and he had been a Phi Delt, and he recommended me. Fraternities and sororities were big at Northwestern, so I became a Phi Delt, not their most enthusiastic member but I lived in the house for two years. It was good. I made friends there. I remember George Hallenbeck, who was a surgeon at Mayo Clinic--he removed President Johnson's appendix--and Howard Packard, who was president of Johnson Wax Company. Most of the boys were of modest academic talents. My best friend, really, was Frank Yoder from Cheyenne, who became a medical doctor.

Riess: As you talk, I think about you as an anthropologist, someone who has been an observer of the scene in which they find themselves. But you weren't yet of that mind.

Foster: I wasn't at all, no. I went to China at the end of my junior year, on a long trip. Even that didn't stimulate me to become an anthropologist. I would have thought it would. I was fascinated by it, but I really didn't know there was such a thing as anthropology. I won't tell you now how I learned about anthropology because there's more to come first.

I was in engineering. That was a terrible mistake. I remember my surveying course, which was my "good" engineering course, I got a B in that. Our first assignment was to take a chain a hundred feet long and go up the gutter of Sheridan Road for about half a mile and then come back again, and the allowable difference was two or three inches, I think. Well, I and the two other members of my team were about three feet off.

The next Monday in class--I had been the head of our survey team that time--the professor said, "Mr. Foster?" I put up my hand, and he said, "Are these figures yours?" I said, "Yes, sir." He said, "You're not kidding, are you?" I said, "No, sir." [laughs] He said, "I've been teaching surveying for thirty years. I've never had an error like this. I didn't imagine it was possible to have an error like this." Well, that sort of gives you a hint as to how my engineering career went.

Riess: Were you devastated?

Foster: Well, I could see the humor, but I just felt--I knew we were in trouble, but I just thought, Oh, hell, things are going to happen, something will happen. But that, as I say, turned out to be my good course. Got a B in that. I remember a course called "graphic statics." I don't know how I stayed in it.

I got through my sophomore year, and I came back in the fall. In my fall term, though, I really went to pieces. I had five units that I just dropped. I remember when I registered that fall--the professors sat in the men's gym, for two days, at the beginning of the fall term and the beginning of the spring term. I was walking along to sign up for a course, and I passed my surveying teacher, and he stood up and he put out his hand and he said, "Mr. Foster, are you still at college?" [mutual laughter]

I could appreciate the humor, but this turned out to be my worst semester. I remember going to see the dean toward the end of the semester. I told him I couldn't be an engineer and I had dropped five units. I said, "I'd just like to go to college and take courses I like." "Well, you can do that," he said--I've often wondered who he was because I'd like to have thanked him. He looked at my record and he said, "You can major in a field and still take almost all the courses you'd like, and I'd recommend that you do it." So I followed his advice and registered in history.

An Anthropology Class

Foster: I enjoyed the history major, but it didn't fit. Toward the end of that semester--this is my fall semester of my junior year--I had a date with a girl named Helen Howell, who came from Ottumwa, and I had dated her occasionally. In high school we had been good friends, and I dated her occasionally at Northwestern. We weren't going steady or anything. But she knew the problems I was having, and she said, "George, I've just taken a wonderful course. It's all about Indians. I know you'd like it. It's called anthropology." I said, "Well, that sounds better than anything I'm taking."

So next spring I registered in anthropology. And she was right! It was all about Indians and others, and I loved it. The thing that persuaded--this was Mel Herskovits, who was a very well known anthropologist, top-notch teacher. And Mickie was in the class, too. I knew she was there, but she didn't know I was there till next fall. I was smitten already.

Well, I knew I was in a good course when Herskovits was talking about the Indians of South America and he was talking about cassava, describing how they pressed the poison out of the cassava root. He said, "Do any of you know anything about it? Have you ever eaten it?" The wheels began going around, and I put up my hand and I said, "tapioca?" And he said, "Right." And that persuaded him that I knew something, I had some potential as an anthropologist.

Riess: I know that we're launched on an important forward story, but I want to step back. Why do you think history didn't have the same kind of interest?

Foster: If I had had a real exciting teacher, it would have been different, but the teachers I had were--well, I actually had only a couple of history courses. One was by a man named Lauer, who was highly rated by students. He taught contemporary thought in history. But I thought he was kind of for the birds. I don't remember what the other history course was, really.

Riess: Do you think that whatever that discipline offered in your engineering studies has been helpful at all?

Foster: No, it was a waste of time. Other than what I learned about life in general.

Riess: Life in general?

Foster: That not everything is exciting or interesting. You have to stick with it and do your best.

[tape interruption]

Foster: I saw my father as a person I could never possibly live up to, never imitate. I could never be the same. I didn't have the talent. But I learned a great deal--. I'm sure he probably told me about tapioca. I remember he described lighter-than-air craft, and particularly the difference between dirigibles with their rigid aluminum structure, and non-rigid blimps. And he also talked about the differences in propulsion for engines and trains and boats, and how airplanes flew.

He talked a lot about engineering, and I learned a great deal from him. I found it interesting. But I didn't have the talent for the mathematics. I never could learn to use a slide rule, for example. I think it was probably--probably the image that a lot of kids have, that they can't live up to their father's example. And I had to get out into a completely different field where I didn't have anyone I had to equal or come close to.

A Trip to China and Japan

Foster: My folks had told me at the time I entered college that sometime during my four years they'd send me to Europe for the summer, to see Europe. Well, the spring of my sophomore year at Northwestern I went to a lecture by a fellow named Upton Close, who was a China correspondent. It was a very exciting lecture. I was turned on by it. During the question period, he casually remarked that every summer he took a small group of people with him on a "cultural expedition" to China. So I went up and talked with him and asked if I could go, if I could talk my parents into it. He said yes, he was delighted to have somebody join the group. So my parents agreed to send me to China with him.

That was a wonderful experience. I remember taking the Milwaukee Railway's "Olympian" from Minneapolis out to Seattle. There I got on the ship to Victoria, spent a day there, continuing on the overnight boat to Vancouver where I boarded the "Empress of Canada." John Morrell & Co. in those days shipped a good deal of meat on the Canadian Pacific to Europe, so my father had influence with the line. He told them that I was on the boat in tourist class, but--to make a long story short, the first mate was assigned the job of taking care of me, and it meant that I could

go on the bridge any time I wanted and climb all over the ship. A wonderful experience.

I seriously considered taking up a seaman's life. But I talked with him and he said, "Don't do it." He said, "You are eight weeks on a trip, and then a week at home, and then you start out for eight weeks again." He said, "It's not a life." I could see it wasn't. But it was a wonderful experience. There were about a dozen of us. I was the only college student. There were several school teachers, and a nurse, and a professor of business from the University of Toledo, a retired principal of a school and his wife from Portland. About fifteen of us, I guess. We got along beautifully.

We stopped in Honolulu, and went on to Yokohama on the boat, continuing to Kobe and then Shanghai, Hong Kong, Manila, and back to Hong Kong. The boat stopped a couple of days in these ports. Finally, after a month, we got off at Shanghai and went overland to Peking, Nanking, and then down through Shantung Peninsula and on an old Japanese boat over to Port Arthur.

That was a wonderful experience. I was with the group until we got to Japan, and then I got mad at the--Upton Close was an interesting fellow, but his wife was a would-be opera singer who was always late, always slow, and I just got fed up with her. I said to him, "I feel I can't go on with you anymore." So we got to Japan, and I left the group and traveled on my own, and that turned out to be much more interesting.

I got to the Inland Sea, to Kyoto and Kobe, and a place called Gifu, where they had a wonderful kind of fishing with cormorants. You'd go out at night--the birds would have a ring around their throat--you know, this type of fishing. I was fascinated. Then I went on to Tokyo and up to Nara. I stayed at Frank Lloyd Wright's [Imperial] hotel, which I knew was a remarkable experience. A very uncomfortable hotel, terrible place. But a gorgeous building.

Riess: Un-comfortable hotel?

Foster: The rooms were very poor. They had narrow windows, and they were hot, and dark. The bathtub was just tiles, rough tiles, impossible to clean. But I knew I was lucky to stay at it.

I remember dinner being served on the roof. One evening at dinner I fell in with an American. I don't recall how I fell in with him. It turned out he knew my cousin, Alice Morrell, who lived in Chicago. He had spent a good deal of time in Japan, he knew the system. So I told him that I was anxious to climb Fuji,

Mount Fujiyama, and he said, "I can help you on that. I'll get you a ticket. I'll call down and make arrangements."

The next night he gave me the tickets, and I got on the train. I was told the station to get off at, where there was a whole series of buses waiting. I just walked down the line of buses, showing my ticket. Finally, one fellow nodded and I got on his bus. After about forty-five minutes we stopped at a little Japanese inn. There must have been something on my ticket saying I was to be let out there. At the inn they were expecting me, and they saw immediately that I didn't speak a word of the language.

I was led into a Japanese room where I took off my shoes and sat down. Presently, a little girl came in with dinner for me, and I ate while she was sitting there all the time, bowing whenever I looked at her. Then, about 9:00 o'clock, somebody came and said--I don't think anyone spoke a word of English, but I knew I was to follow. Here was a horse with a man, and a 12-year-old boy who carried an ordinary Japanese lantern with a candle in it.

So we started up. I rode up the side of Fuji to about the 10,000-foot mark. Every half hour we'd stop at a wayside inn and go in and drink tea and sit around a fire. I carried a staff I had been given, and each way station would stamp the stamp of that station on the staff. At about the 10,000-foot level, at perhaps 2:30 in the morning, we got as far as the horse could go, so the kid took the horse on back, and I climbed on foot the rest of the way, with this guide, getting to the top at about 4:30 or so, and to the inn, if you could call it that, where I lay down on the floor with some comforters, and there was a little fire.

I had just gone to sleep when I was wakened by the guide. He brought me a can of pineapple, which he opened, which was my breakfast. It was just dawn, and he made me walk around the entire crater. It was a wonderful experience. I remember the sun coming up under the clouds cast a curious reverse shadow. I had never seen anything like it.

Well, the time came to go down. He had a series of what in Spanish are called *alpargatas*, reed sandals. He tied a couple around each of my big shoes, and we went to the edge of the mountain and just jumped. Each step would take us fifty or more feet, as we slid down through this volcanic sand.

Riess: Please describe this better. I can't imagine.

Foster: Well, I'd think that so many people doing it would have worn the mountain away by now. I obviously had no clothes for this kind of mountain climbing. But he had seven or eight sandals, or pairs of

sandals in his pack, and my feet were so big compared to the Japanese that he tied two around each shoe to protect it. And then we'd just take these giant steps and slide down. Each step would slide us fifty feet or more.

Halfway down, he decided I had worn out those sandals, so he stopped me and put on new ones. I remember it took an hour and forty minutes to go all the way down. It had taken eight or nine hours to go up. I was much impressed.

Riess: That has a dreamlike quality, the whole expedition.

Foster: The whole trip, yes.

Riess: You didn't speak Japanese. How did you manage?

Foster: Well, I guess I knew enough about the places I wanted to go. I can't remember how I knew. I knew I wanted to go to Gifu. I don't really know how I decided, but I was glad I did. It gave me a sense of ability to get on on my own, by myself.

Riess: What else did you do before coming back?

Foster: I had my ticket. We were all on the same boat, the "Empress of Japan" going back, so I was with old friends on the return journey.

[tape interruption]

Lessons of Geology and of Scouting

Riess: I'd like to hear more about your class in geology at Harvard.

Foster: Well, I never had anything like it: the idea of synclines and geosynclines. I knew sandstone, for example. Limestone, igneous rock. But to see it all--the area around Boston is wonderful for geology: you get the glaciation; you get the seacoast; you've got mountains; you've got volcanic intrusions. It's a marvelous location to study geology, and it just excited me tremendously. I loved it.

Kirtley Mather was a terrific lecturer, but he was wrong on one thing: I remember how he poo-pooed the idea of continental drift. He said, "That's just an illusion. It couldn't possibly happen." Well, that's what all contemporary geology is based on now.

Riess: I guess plate tectonics wasn't known.

Foster: No, it wasn't. Well, it was dreamed about, but only dreamed about. I've often wondered if he accepted it ultimately or not. He was very well known. He was one of the top professors at the school.

Riess: And you mentioned being on horseback, which made me wonder about your outdoors experience in Ottumwa, horses and all.

Foster: There were no horses. As a scout, I did a lot of camping from the age--I became a Boy Scout on my twelfth birthday and was active until I was fifteen. I didn't complete my Eagle scouting work until I was seventeen, when I decided I'd better wind it up.

Scouting was very important. We had a wonderful scout leader of all the troops in town. Rex Gary was his name. He had been gassed in the First World War, and he'd had to adopt an outdoor life to live, and he'd had the good luck to get into scouting. He was just made to lead scouts, and he had some wonderful training courses for us--with some he was lucky he didn't lose a scout or two. I remember he had one course that went every weekend for the whole spring. At one point, we had to fell trees and build rafts and cross the Des Moines River in flood.

Riess: Oh, really!

Foster: One of the three fellows on our raft couldn't swim. We got to the opposite shore, but the current was so swift the raft kept bumping the bank, so we couldn't get off. Then the raft started to come apart. I was really worried, but we got through all right. I think we learned a lot about independence through that course.

I was the youngest one of my group. I was very proud of this because John Mahon and a boy named John Dunning were the top scouts. They expected to have the best raft, they built it, and their raft sank [chuckling]. It made us feel pretty good that we were getting across the river, while these top scouts were trying to raise their sunken raft. But it was a very tough course.

One important lesson I learned--there was a fellow named Ben Cavins. He was from a really poor family. I didn't know what poverty meant. I remember we were camping in the winter and we didn't have enough blankets. He said, "Put your head under the blankets. That warms you up." He'd never had enough blankets at home. His mother had taught him that.

On one of our expeditions we had to hike fourteen miles after school on Friday on a muddy road, and it seemed awful, and I was just about to throw in the towel, and Ben said, "Oh, let's keep on a little longer." I was ready to throw in the towel again, and Ben said, "Let's keep on a little longer." We finally got to where we were going. I've always been very grateful to Ben. He never went to college. Instead he joined the navy and went to Hawaii, where he contracted spinal meningitis and died. But he taught me a very important lesson, and that was a very good lesson, too, because I began to realize that it made no difference what your background was, if you had good quality you had something to contribute.

I've often thought of him.

In 1926 when I joined the scouts, or '25, the movement was only about twelve or fifteen years old. Most of the troops were associated with churches. The American Legion had Troop 17; the First Presbyterian Church had Troop 19. You joined the troop not of your church but that had your colleagues, your neighbors. There was good competition, but it was a friendly competition.

My next experience with scouting was after the war when my son, Jeremy, was a scout in Washington, D.C. I felt the scouting movement had gone all to pot then. Scouts were more interested in radio than in scouting, and in one-upmanship and making smart cracks, and talking about bigger jamborees every year.

Riess: You said the camping experience was put to good use?

Foster: Doing field work in Mexico, yes. When I first went on horseback in Mexico, I was not the least worried about my ability to do what I was going to do. I knew I could do it.

More on Father's Questioning of the Conventional Wisdom

[Interview 2: November 17, 1998] ##

Foster: There are several points I'd like to fill in on last week's session about my father and his mind. As I said, he was politically conservative, but he was very open to ideas professionally. I've got some examples that have always struck me. In those days, when I was a kid, there was a lot of home canning. And the glass jars, the Mason jars, had to be sterilized by pouring boiling water in them, and some of them would break. It was widely believed that if you put a silver spoon in a glass

jar, the spoon would absorb the heat, and the jar would not break. That was de rigueur for all home women canners.

My father, after his engineering training at Pennsylvania, reasoned that no silver spoon could absorb enough heat to make a difference, so he collected a dozen Mason jars and put silver spoons in half of them and nothing in the other half and poured boiling water in all of them. There was no difference. None of them broke.

Another time he told me how at the time he was in college [1906-1908], the meat packing industry lacked any scientific basis at all. He said it was widely believed that there were two kinds of heat: heat from burning wood and other fossil fuels, and body heat--"animal heat," it was called. So when hogs were slaughtered, rather than putting them in a freezer [a refrigerated room that could hold a thousand hogs] to chill them rapidly, they were chilled slowly. The plant had three different freezers. After the animals were killed they were put into a room of perhaps fifty degrees, and after being left there for twelve or twenty-four hours, they'd go to a room where it was perhaps forty degrees, and only after they'd been there for another number of hours were they really frozen or chilled to the point where they could be cut into ham and bacon.

He said he figured there was no such thing as animal heat, that heat was heat. "If I hadn't been boss's son," he said, "I couldn't have gotten away with this, but I decided I'd try putting the recently-killed carcasses into the sharp freezer [the coldest room] right away." He said, "Of course, it worked. We cut down on a tremendous amount of loss."

On another occasion, he was telling me how--you probably don't remember--in the old days we used to see refrigerator freight cars going across the country. They'd be stopped at places like Sacramento, Reno, Cheyenne, and Omaha, with the trap doors at the end of the cars opened. Those were the ice bunkers. The refrigerator cars carrying meat had 5,000 pounds of ice in each of those bunkers and 40,000 pounds of frozen meat. The train would go about 500 miles a day, then they'd stop for some hours and slowly empty and re-ice these cars.

My father said, "I got to thinking that it's ridiculous to think that 5,000 pounds of ice is going to have much effect on 40,000 pounds of frozen meat, so we started them out for the West Coast"--a trip of about five days, I think, then--"and regardless of whether there was ice or not in the bunkers, we kept the train moving." He said, "Of course, we suffered a lot less loss than we would have otherwise have suffered."

So I have always admired his spirit of questioning the status quo, the conventional wisdom.

Riess: Yes. And he would share his thinking with you.

Foster: He talked to me a lot about it, yes, and I was interested in it. I forgot to mention that I read the *Scientific American* religiously. He had subscribed to it since he was a boy. I remember he told me when--after fifty years, the *Scientific American*, at least formerly, had a few notes about "in our edition fifty years ago"--and when he began reading the "fifty years ago," which he remembered the originals of, he realized he was getting along in years. He enjoyed that.

I always found him very interesting. He was a widely and well-read man, of a certain type. He didn't read English novels or things like that, but he was well informed about the world.

Riess: And you had the sense of his wishing that he might have been a scientist.

Foster: He would have liked to have been a professor, he said. And my mother said it, too, that "it's too bad he wasn't able to be a professor. He would have been much happier that way than running a business."

Riess: Being a professor means he had a gift for teaching? Why say, "be a professor" rather than "be a scientist"?

Foster: Well, for me, a scientist is a professor, and a professor is a scientist. Teaching and research are intimately linked. I know there are scientists, in the hard sciences, who work in research laboratories. Bell Laboratories, for example, and General Electric in Schenectady. But my father thought of it as being a professor--he would have loved that. [with emotion] He influenced me profoundly.

II STUDYING ANTHROPOLOGY

Melville Herskovits, Defining Anthropology

Riess: You said that Melville Herskovits at Northwestern was your first introduction to anthropology?

Foster: Yes, he was. I hardly knew the term before than. I told you last week how I was floundering around at Northwestern, and one of the girls I had gone with in high school told me about a course she was taking and that it was called anthropology, and it was all about Indians. And that's how I got into anthropology--I think chance has been the leitmotif of my whole life.

Riess: The first definition of anthropology for you was that it was "all about Indians?"

Foster: That was my first definition. I had been interested in the out-of-doors, and I had read a lot of books about the West. I realize I read a good many travel books, *The Royal Road to Romance*, Richard Halliburton, came out when I was about thirteen or fourteen [published 1925]. That turned me on.

And there was another fellow called Harry [A.] Franck, whom my father read widely. He wrote books with titles like *Vagabonding Down the Andes*. He was a terrible writer. You knew how his feet felt, but you didn't know what he had seen, or what the people he met felt or thought. I don't know why my father liked him. I tried reading several of his books, but I just couldn't take him.

I've always been interested in how other people are and how they live. I mentioned my trip to Porto Rico as the great experience of my life up to that time. And the trip to China in 1933, again, I just devoured every moment of it. But still I didn't think of becoming an anthropologist. I don't know why.

Riess: And Herskovits convinced you that you could become an anthropologist?

Foster: Yes, it was his personal magnetism and ability as a teacher. The first course, in the spring of 1934, the spring of my junior year, was largely cultural anthropology. But he set the field. He told us about archaeology and linguistics and biological anthropology. He was a cultural anthropologist himself, but he had also worked with the physical anthropology of Negroes--Africa was his great field, of course. He was one of the later students of Franz Boas, who had trained both Lowie and Kroeber also. So you might say that I was trained as a third-generation Boasian, as were most of the anthropologists in this country at that time.

Riess: Earlier controversies about race and eugenics, were they still live issues in cultural anthropology when you started your work?

Foster: By the time I got into anthropology, and I think for about ten or fifteen years before that, the theme that all our professors hammered on was that race, language and culture are all separate, that any race is capable of carrying any culture, and speaking any language, that there's no such thing as an organic relationship between race and language and culture. Boas hammered on that.

Cultural relativism, or biological relativism, was also central then, at least in my experience. That is, Herskovits told us, and I think it was generally what was being taught in school, that any apparent differences in the ability of the races was due to cultural background and not to race. So it was a sound and I think very good way to start us out.

Riess: Did you know that there was an issue there?

Foster: Yes. We--let's see, Madison Grant wrote *The Passing of the Great Race* [1916]. That was a book my dad had read, so I saw it at home. For a while, I thought he had read all the wrong books on anthropology [chuckling]. Yes, we were very much aware of the eugenics movement, at least I was. But it was not a basic part of our training, other than to have it rejected.

Riess: What were your textbooks?

Foster: Well, for physical anthropology we read [Earnest Albert] Hooton, *Up from the Ape* [1931]. And we also read parts of Kroeber's 1922 *Anthropology*. I don't remember what we read in our introductory course. I believe we read--funny, I remember what some of the books were here at Cal, but I can't remember my undergraduate books.

Riess: Did Herskovits write a text?

Foster: Yes, but he wrote it after I left. His text, *Man and His Works* [Knopf], came out in 1948.

Riess: Hans E. Panofsky, who in 1979 was curator of the Herskovits library at Northwestern, wrote this about Herskovits:

Herskovits spent his entire working life at Northwestern University. He came here in 1927 and taught here until his death in 1963. He was one of the first academics interested in Africa. In theoretical matters, there was his cultural relativism, which is probably his main theoretical contribution, adopted from psychology most of all. He was not foremost a theorist. He was interested in the whole humanistic range of anthropology, be it music, culture, the fine arts, and so on. But it would not be correct to say that he founded a school. I think that with his death the direction taken by the anthropology changed considerably, and the sort of humanistic emphasis was considerably lessened [In *On Going Beyond Kinship, Sex and the Tribe*, Scientific Publishers, Belgium, 1979, pp. 69-70].

Foster: That's a fair statement of him.

Riess: "Cultural relativism...adopted from psychology most of all"--what does that mean?

Foster: I don't know what is meant by that, "adopted from psychology." I didn't realize, if Herskovits in fact did draw on the psychology, that his ideas of cultural relativism came from psychology. And I doubt that was the main impetus. It was pretty general among anthropologists, beginning with Boas, as I said. Lowie and Kroeber all had the same view.

Riess: And "not foremost a theorist?" What does that mean?

Foster: The dominant motif in anthropology in those days, and for a great many years thereafter, was the natural history idea that there are all these facts out there and we're going to lose them if we don't gather them, and we saw our job, in ethnology, at least, to describe as many so-called primitive peoples as possible, as rapidly as possible. Boas, who was the person who set that theme, said "theory can come later," which was an unfortunate way to put it because if you gather your data with theory in mind, you get much more relevant data. Certainly there was little emphasis at Northwestern, and at Berkeley, on what I would call theory, or else I was so stupid that it went over my head. I didn't know.

I think anthropologists work very much like [Charles] Darwin, who gathered masses of data. It's a natural history approach rather than a laboratory approach. Darwin was lucky in that he had a triggering experience in the Galápagos that brought it all together for him. That's the way anthropology works. Most of us don't have a Darwinian experience, but we'd like to.

Riess: Did you declare a major in anthropology in your junior year, or what?

Foster: At the beginning of my senior year, I walked into the gym to register and I didn't know what my major was going to be. I thought it was going to be anthropology, but Herskovits was not yet back from a summer of research in Haiti. Just at the last minute he got back, and I registered as an anthropology major.

The dean had to stretch the rules a bit to get me a major, because it wasn't possible to take enough anthropology to have a legitimate major, but I had taken a history course from Lauer, and a couple of psychology courses, one from Maurer, and another general psychology course, which was the dullest course I'd ever had, and a sociology course, which turned me off from sociology as well. So these courses were cobbled together. I took a B.S. degree in anthropology because of the engineering units that I had--I didn't have enough liberal arts units to take a B.A. degree. I'm probably the only anthropologist in the country with a B.S. degree who is not a biological anthropologist.

Riess: That reminds me of Panofsky's remarks referring to "the whole range of anthropology: music, culture, the fine arts, and so on."

Foster: Herskovits certainly was very fond of music. He was a wonderful drummer. He gave a wonderful course on primitive art. He was very sensitive to the aesthetic side of culture, and he communicated his ideas very well to his students.

When I came to Berkeley I found the greatest difference was the emphasis on facts. We had to memorize thousands of facts. For a while it seemed very dull, very elemental, but in the long run I think it was good that I had both experiences. Comparing my Herskovits notes with those of Lowie and Kroeber, each lecture was very thin, the notes I had from Herskovits, but I remember very well the lectures and the way he--he had an easy way of talking. He'd ask students questions--I told you about his question about cassava, and my answer. The class had about forty students. It was an ideal size for a learning experience.

Riess: When you say "thin," you mean as in not masses of material?

Foster: Not masses of factual data is what I meant. In Lowie's course, I remember, I was so busy writing the facts as they tumbled out that I was astonished when the fifty minutes had gone by. In Herskovits's class, I took relatively few notes but probably remember more of what he said because I could concentrate on the ideas he was discussing.

Riess: To the degree that he was more aware of the humanistic aspects, art, music, has that given you a different view? Has that been a big influence?

Foster: No, I don't think it has. I've always been much interested in material culture, but art as such I've not specialized in, and music--I can't drum, I can't sing, I can't do anything artistic.

Riess: Was there an important museum at Northwestern when you were there?

Foster: Not at all. The Field Museum in Chicago was the museum, which I of course had gone to from my childhood. Every time we'd come to Chicago, I'd go to the Field Museum. I couldn't imagine going to Chicago without going to the Field Museum.

Riess: Did Herskovits's work in Africa interest you?

Foster: Yes. For a while, I thought I'd be going to Africa. My senior year at Northwestern, the year I was deeply in anthropology, there was a British physical anthropologist named Jack Trevor, who was Herskovits's first postdoc. Jack and I became good friends. We planned to do field work in the Great Rift Valley of East Africa. He would do the physical anthropology, and I would do the cultural anthropology. But it never came to pass.

And when I came to Berkeley, without any African influence [here] at all, I turned to Mexico, and I'm very happy that I did. I didn't get to Africa, really, until a great many years later. I've seen quite a bit of Africa now, through my consulting. But I find Mexico tops, very interesting.

Graduate Work at Berkeley

Riess: How did you decide where you wanted to go to for graduate work?

Foster: I came to California for two reasons: one, Kroeber and Lowie were here, and they were in my opinion tops at that time. And two, I liked the idea of getting as far away from the Midwest and the East as I could. I just loved--I had been to California. On my

trip back from China, I swung down from Vancouver through San Francisco. Spent a couple of days, including a Grey Line tour of the East Bay, and saw the university. It never occurred to me that I'd ever be here. And then I went on down to Santa Monica and spent a couple of days with my father's sister, Annie, who had settled there with her husband and kids.

There were very few choices for graduate work at that time: Columbia, where Herskovits and Kroeber and Lowie had all studied, was still going strong, but Boas had just retired, and Herskovits wanted me to go to Wisconsin to study with Ralph Linton, who would have been a very good man to study with, but he left Wisconsin a year or two later and went to Columbia.

Harvard was a possibility, but having been there, I didn't want to go to Harvard. Pennsylvania was another possibility, and Chicago was the third. And having been in Chicago for three years, I wasn't interested in staying in Chicago. So it was just kind of natural to come to California.

There was no anthropology taught at UCLA then. Anthropology was taught at the University of Washington, and Minneapolis, the University of Minnesota, had a bit of anthropology, but I don't think there was anyplace other than California west of the Mississippi where one would seriously think of taking a doctorate.

Riess: It's so interesting that it was such a new academic profession.

Foster: I don't suppose there were two hundred anthropologists in the whole country then.

Riess: So you can define yourself. But as you talk about it, you define it by traditions.

Foster: Everybody, regardless of the area in which he or she specialized, took the same courses. That was true at Berkeley when I came; it was true at Harvard, Sherry [S.L.] Washburn tells me. When I think of the graduate students in Berkeley--with Kroeber and Lowie each giving a seminar both semesters, that meant only four seminars a year, and if you had a dozen graduate students you were with the same ones the whole way. It meant that we had a feeling, we understood what the other people were doing. Even though we weren't doing it ourselves, we'd read the same books.

I think it was a nice period to start anthropology: the field was being defined, the specialties were being drawn more sharply, but the fact that we all had exactly the same training, or essentially the same training, meant we had a feeling for the coherence of the field, which is smashed to smithereens today--

there's no sense of the unity of the field. The reason I think that the field was unified was that, regardless of what field we were in, we were all concerned with origin and distribution and development. History. Anthropology was history at that time.

Riess: Coming to Berkeley you said you "knew" Kroeber and Lowie. That meant that you had been reading their texts?

Foster: Yes, and Herskovits talked about them a great deal. He regarded them very highly.

Riess: Somewhere, perhaps in one of your descriptions, I read that the coursework included the history of anthropology. In other words, you studied the greats, like [Bronislaw] Malinowski. What would be the reason that you would be studying the individuals in the field?

Foster: Well, for their methodological approaches, the kind of data they found, and the ideas they had. We didn't view it as history. It was contemporary at that time. Malinowski was a younger man than Kroeber, about the same age as Lowie, I believe. He was still very active. There were very few anthropologists other than Boas himself who weren't still active, so we were studying contemporary anthropology. Only in retrospect does it become history.

Riess: Yes, that's right. But was it also a search for theories?

Foster: Yes, in terms of their theoretical points of view. But the kinds of theories were so very different from what we're concerned with now. The nature of totemism, for example. Lowie wrote his dissertation on totemism. That was an important thing at that time. [Clark] Wissler's "age-area complex" was another theory. Historical reconstruction was a methodology that was very important. [Edward] Sapir was the leader in that. And independent invention versus diffusion. Anthropology concerned itself to an excess with traits, culture traits.

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Foster: When we would identify a culture trait, the question was whether its presence was due to independent invention or to diffusion. I'd say that was really the dominant theory that I worked with, the principal theoretical type of question. That led into my interest in acculturation, which was Herskovits's, one of his principal, early themes. He and [Robert] Redfield and Linton drew up a memorandum in 1935, published in *American Anthropologist* in 1936, on acculturation: what it was and what it implied, the kinds of data one must gather. That was very influential in my thinking

when I went to Spain to get the Spanish background of Spanish American culture.

Riess: What does "acculturation" mean?

Foster: Acculturation is defined as the processes involved and the results when two different cultures come together, how they borrow from each other. (I don't know why we use the word "borrow"--"steal" would be more accurate.) Why we take elements from the other; how we integrate them in our own culture; how cultures change as a consequence of contact; and the power relationships, the dominant and the subordinate ones. During my formative years as a student, and the first fifteen years of my professional life, I'd say acculturation was the single biggest theoretical area that anthropologists were concerned with. It was a major topic.

Riess: And it was studied historically?

Foster: Yes. Historically and through the literature. It was a static way of studying process. It dealt with traits, the origin and diffusion of traits. And we missed a great deal. In our book on long-range studies we discussed the shortcomings of the typical acculturation approach.¹

Riess: Did you have doubts, or were you troubled by what you were learning in your graduate school years?

Foster: Doubts as to whether I should be in the field?

Riess: No. Doubts as to whether--did you have any idea that something major was being missed?

Foster: I was so excited by everything I was exposed to, I don't recall anything like that, any doubts at all. I was so delighted to find something I could devote myself to with enthusiasm full time. And I bought the whole picture as it was presented. I was not a very discriminating young man.

Riess: A question: at that time had you any experience with psychoanalysis?

Foster: No.

Riess: Did you find the concepts agreeable or alien or anything?

¹Foster, *Long-Term Field Research in Social Anthropology*, 1979.

Foster: I was not really exposed to it in my graduate work. Kroeber had been a practicing analyst for several years, but he never talked about it in lectures, and I didn't really learn much about Freud until later. When we lived in Washington, I remember Mickie and I took a course from Karen Horney, who was lecturing in Washington, and I began learning about it then. But when I was in graduate school, I couldn't have told you beans about Freud.

In the spring of 1935, my last semester at Northwestern, I wrote a letter to Kroeber and said I'd like to come to Berkeley and study anthropology. I got a letter back from Lowie, saying that he was chair that year and was handling admissions, and he urged me not to consider coming. There were no jobs, and it was just a dead-end field. I wrote back and said, "Well, I'd like to come anyway." So he replied and kind of grudgingly accepted me, I don't know why. There were no exams or anything. I'm sure Herskovits gave me a good letter, and I expect it was on the basis of that that I was accepted because my academic record was foul, though I did manage to graduate with honors from Northwestern. I don't know how I did that.

Fellow Graduate Students

Foster: In any event, I arrived about the middle of August and found there were two other beginning students. One was Walter Goldschmidt, who has been at UCLA for years, and the other was a young man named Tommy Garth. Tommy was a nice fellow, about my age, about twenty-one, I guess, whose father I think was a psychologist at Colorado. He was kind of, you might say, a legacy, at least that's what we were led to believe, that Kroeber felt he had to admit him because of his father's tie with Kroeber.

Wally went on and became an eminent anthropologist, but Tommy disappeared early in the game. So there were really only the two of us. And Bob [Robert F.] Heizer was here. He was a senior. At that point, he knew more anthropology than I ever knew, I think. He had been out digging in Alaska and Nevada, as well as the Bay Area shellmounds. He was already, as he was all his life, an omnivorous anthropologist.

Riess: Sounds more like an archaeologist.

Foster: He was an archaeologist, but he'd read everything in cultural anthropology as well. There was [Margaret] Lantis, there was Katharine Luomala, Homer Barnett, Omer Stewart, Frank Essene.

They're the ones I remember immediately and best, as being already established as graduate students.

Riess: But the incoming class was only three people?

Foster: Yes. That was about average for several years, I think. The small scale of the graduate program was amazing. There were only about ten or at most twelve graduate students in residence at any time. And half of those were teaching assistants, and the other half used the teaching assistants' room for their own room, so we were all kind of piled in together in the old tin shack, which is on the site of the Hertz Hall now, the school of music.

Effectively, Kroeber and Lowie were the two people that gave the graduate courses, and they didn't have much time to devote to graduate students, they were very busy in their research and professional matters. I don't remember ever sitting down in either one's office until after I passed my qualifying examination. Just incredible. We'd go in at the beginning of the semester and stand up in front of the professor's desk and talk for perhaps ten minutes about what we were going to take, and then we'd go out. We'd see them in classes, but there was none of this easy give-and-take that has become, happily, much more a part of the university system.

Riess: If anthropology is so much a tradition, handed down from Boas or Kroeber and Lowie to the next generation of students, they were really handing you the mantle. You were chosen, you and Wally, to carry the mantle.

Foster: We sort of chose ourselves, I suppose. Fortunately, unlike some teachers, neither Kroeber nor Lowie ever thought in terms of having disciples. I don't even know who was my principal advisor. When I came back in 1953, I was astonished to find that professors had students who were their own, and that it was a matter of one's academic record who your graduate students were. As I say to this day, I can't recall whether Kroeber or Lowie was the first person to sign my dissertation.

Riess: That's interesting.

Do you think it is true that at that time there was more equality for women in the profession? And more opportunities for them?

Foster: I think that was true of the training but not in job openings. I don't think Kroeber would ever have given a job in the department to a woman. He never did, in any event. Lowie was much more open to women than Kroeber. Kroeber was fair with them. He didn't

object to giving them doctorates, but he would never go out on a limb and nominate them for a job.

But there were very distinguished women who preceded me. I suppose the ones that were best known were--Katherine Luomala was one of the leading folklorists in the whole world at that time. Margaret Lantis was a splendid ethnographer and still is. She was an Alaskan expert. Isabel Kelly was a marvelous person who lived in Mexico most of her adult life. Let's see. Who were the other ones? Dorothy Demetrocopoulou Lee was always Lowie's favorite. She was of Greek background. She was at the Palmer School in Detroit, I think it is, most of her adult life. I never met her.

Riess: Why are women so good at anthropology?

Foster: I think they've got just as many brain cells as men. It's kind of a pointless question to me. Why shouldn't they be as good?

I think the nature of anthropology is such that any discrimination against them in anthropology might have been due to the fact that there were doubts as to whether they were tough enough to take some of the field experiences. But Martha Matilda Stevenson in the late years of the 19th century and Margaret Mead and Hortense Powdermaker very quickly showed that they could do anything the men could do.

The reason there weren't more women in the field was the basic university prejudice against having women on the faculty, the feeling that a real professor had to be a man.

Carl Sauer

Riess: You talked about Kroeber and Lowie. What about the influence of Carl Sauer?

Foster: He was kind of an intimidating person to a lot of us. He sat and puffed his pipe continually, and growled at us, but he was a very stimulating teacher, and everybody in anthropology took one or more seminars from him. I think I took three altogether. He was on almost all of our committees. He was on both my oral committee and my dissertation committee.

Riess: What did he teach you?

Foster: What I remember particularly is the origin of domesticated plants and animals. That's something that has given me great pleasure

all of my life, to travel around the world and be exposed to exotic foods and know where they came from. For example, I think it's becoming much more common knowledge now, but for many years if you ate a papaya, you had no idea where it started out its life, or a mango, or wheat or oats or barley or corn or tomatoes or chocolate.

Riess: Did he do original work in that? Was his class based on his research?

Foster: A lot of it was. He worked in Mexico a good deal. Come to think about it, I don't know where he did his major fieldwork.

Riess: I've always been interested in thinking about how someone had to perish in the experiments to select among the roots and mushrooms and berries and so on that we eat now.

Foster: I think probably a lot of people died, and presently people caught on to the fact that if you ate this red fruit, delicious as it looked, you weren't going to be around for long to enjoy it. But I don't know how--I don't think we ever discussed how primitive people began to sample the apples, oranges.

Riess: Tasting the fruit is a biblical image. I wondered how religion, how one's own religious bias, was dealt with in your classes? Did that seem ever to be an issue?

Foster: I don't recall it was ever an issue. I don't recall religion ever entering our discussions on these topics. I think most of us were pretty agnostic. Kroeber, in his course on the origin of culture, which was a fantastic, wide-ranging course, beginning with the Mesopotamians and Egypt and Greece and the Chinese, the Indians, Hindus--he talked about religion, of course, but I don't recall anything particularly original on this topic that came out of those lectures.

Riess: In a way the first culture contacts were missionary contacts. Before anthropologists arrived, missionaries had put down roots. So you had to deal with the impact of the missionaries.

Foster: Yes. The missionaries in Africa and Oceania, they wrote some of the first good ethnographies. [Henri A.] Junod's *Life of a South African Tribe* [1913], one of the basic African texts that we used, was written by a Swiss missionary, I believe he was. But missionaries were kind of looked down upon by the anthropologists. We felt very superior to them.

Seminars from Kroeber and Lowie

Riess: It's remarkable to me that Kroeber and Lowie could continue to teach what looked like the same classes year after year.

Foster: I think they must have changed a good deal.

Riess: They brought new material from their own field research?

Foster: They were not doing much field research by the time I came. That was the interesting thing about so much anthropology in those days. The anthropologist as a young man or a young woman would go out and spend a series of summers or maybe two or three years doing research, and then come back and live on that the rest of their professional lives.

Lowie, for example, did the bulk of his fieldwork between 1910 and 1916, and those were the years in which he spent several months each summer with the Crow Indians. Kroeber, his research lasted over many more years because he was right in the heart of it in California. He came here about 1901, I believe, and I think the last time he was in the field he must have been more than eighty years old.

In 1935, when I came, Kroeber was sixty and Lowie was fifty-three. They were not doing active field research then, but they were still writing a lot and they were very active intellectually at that time. But the picture's changed dramatically. It's so easy to go to the field now. Here I am, at the age of eighty-five, going down to Mexico for Christmas for the third time this year--not that I'm going to get anything really important out of it, but I'll bet some things will be interesting.

And Elizabeth Colson, who's about eighty or so, this last spring was in Zambia for six weeks or so, living in a tent and doing real tough fieldwork. You just can't imagine this from that earlier group of people. I think that's where we have the advantage over them, in the stimulation of our research materials, in that we can see the changes that have come about. We can talk about change from having observed it.

When the acculturation theory was developed, people were looking for a base line, which was presumed to have been static, the condition of a group before western civilization came upon it and began bringing about all these changes. But a continuing field experience is much more important today, I think, than it was to anthropologists in those days. All societies are changing

much more rapidly now than during the early years of the present century.

Lowie would give a seminar called "The History of Theory." He would take a different person every year. One semester it was Malinowski, another year it was a German anthropologist who was interested in economics, Thurnwald--I can't think of his first name. So he was using this approach to gather data to write a book on the history of anthropology. His *History of Ethnological Theory* (1937) was the result of several years of courses and seminars, in which the students would read widely and present papers. He would guide us in our research, and then he would do a chapter for the book. It was a good way to do a book.

Kroeber--I remember a seminar that he gave us on the Japanese language, the only seminar I had in linguistics. We met several times, but then he left us on our own, and we had to turn in a final paper.

Riess: How in the world did you do that?

Foster: I don't know how we did it, but I learned enough so that when Mickie and I were doing our fieldwork in Veracruz, my doctoral dissertation, we were able to gather sufficient data to do a fairly passable grammar of the language. She'd had less training than I had in linguistics. But in those days, we assumed we could do anything. We were trained to do anything.

Riess: That's key, isn't it?

Foster: It is.

Riess: I mean, this was just book learning about Japanese language.

Foster: Yes. We never had an informant or anything. I learned the international phonetic alphabet that everyone used to record foreign languages, non-written languages. [Leonard] Bloomfield, who was talking about phonemes then, had written his book, I forget what it was called [*Introduction to the Study of Language*, 1933], in about 1932, a major book on language, the best to date and for a good many years after. But he was never mentioned by Kroeber or other local anthropologists.

Riess: This idea that you could do anything was a byproduct of the way Kroeber and Lowie taught, are you saying?

Foster: I think it was a byproduct of the way almost everyone taught anthropology in those days. We were taught that we would go to the field, and we'd have a research topic which we should

concentrate on, but we should take advantage of any opportunities that came, and if we saw something that was more interesting than what we had planned to study, drop what we came out to do and take up the new. I think that's very good advice. I've always advised my students to do the same thing. Go where the gold is.

The Isolation of Berkeley, the Cultural Remove

Riess: Perhaps you would not have gotten that from Columbia or Chicago? Do you think this was a hallmark of California?

Foster: I don't really know. I think students from Columbia and Harvard were doing the same thing. Probably not from Chicago, where they were pretty rigidly trained in British social anthropology.

Riess: Is there something that you might say about this university on the edge, this westernmost university, a sense of possibility?

Foster: The striking thing about my early days here was how isolated the West was. There were airplanes: United Airlines flew to New York, a trip of twenty hours. But because of the expense, few people flew. It was sixty hours by train to Chicago, three nights and two days. To Seattle, I think it was thirty-six hours. Los Angeles, where there was no anthropology, was twelve hours on the train. So no student ever went to a meeting until he had his degree. Kroeber and Lowie missed Christmas with their families all of their lives, because if there were meetings in Boston or Cambridge or Washington they'd need four days to get there and four days to get back. And they went religiously.

When we came to Berkeley, we felt we were in a new country, just a new world, isolated. We knew there was anthropology clear back in the East, where many of us had come from, but our concern was the here and now, and the isolation was a wonderful feeling.

Riess: What are the pluses and minuses about that? You talk about the chance to invent yourself. Was that more possible here?

Foster: No, I think--in those days, at least--I think there was a personality of anthropologists. I think we were all kind of clamoring to get out of our own cultures.

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Riess: You say you were all clamoring to get out of your own cultures?

Foster: All my life, the first question anthropologists have asked each other when they meet is, "Where did you do your fieldwork?" Often you know, of course, but you ask. The field is the common bond that we all have, whether we're archaeologists or linguists or cultural anthropologists: comparing notes, how the peoples we study are similar, how they're different. That's unendingly fascinating to all of us, I think. I found that when I was lecturing in class, if I felt the students were dozing I'd just start talking about my field experiences, and the class would snap to, and they'd clamor for more of it. I think we've all had that experience.

Riess: And you are also saying that a hallmark of anthropologists is that they were eager to get out of their own cultures?

Foster: Yes.

In those days, I think there was very little awareness of cultural difference among the population at large. The term "culture" now is widely accepted. You talk about the cultures, business cultures, office cultures. It's a badly overworked word. But in those days there was none of that. We talk about culture shock now, we have for the last forty years or more. So anthropology has intruded upon the popular mind to a fantastic degree, I think, in comparison to what it was when I first fell into the field.

Riess: You wouldn't have talked about Ottumwa as a culture.

Foster: No. It wouldn't have occurred to me. That's the last thing on earth I would have wanted to have done. I was in anthropology because I wanted to know how the Mexicans or the Africans or some other people who were completely different from my society lived: what they thought, how they worked, how they constructed their lives.

Now, of course, a great deal of anthropology is done on American society, American institutions, but I think the anthropologists who do that are much better prepared than we would have been, because even though many of them have not worked outside of their own country--when you get into a medical school you're in a culture that's a foreign culture, and I think we realize how these professional cultures differ. And we've got ethnic cultures, of course--I think it's very important that we do study American society and all its variants today.

Riess: I wonder, do you have to completely understand the culture you come from before you start asking questions in another culture or

community? This is what I meant when I was asking about Ottumwa as a culture.

Foster: I think it would be better, but I certainly was not aware, at the time, of my own assumptions influencing my theoretical conclusions.

Riess: You could be neutral observers.

Foster: Yes, that was our goal, and we assumed we were. We learned later that we were not as neutral as we thought, but I think we did a pretty good job at that.

Riess: Yours will be a particularly interesting history because of the differences between the beginning and the end of your profession.

Foster: Oh, yes. It has changed beyond belief.

Rote Learning, Facts

Riess: I believe you made the comment that Berkeley was a rigorous department, perhaps to a fault, but largely ignoring new developments such as British social anthropology, culture and personality explorations at Yale and Columbia, American social approach.

Foster: It was, it was an old-fashioned department when I came here. As I say, Kroeber was sixty and Lowie was fifty-three, and even in the best minds you don't find a great deal of originality after those ages.

You spotted something that I never thought of, the fact that they had offered the same courses year after year. As to how much they changed them, I don't know. I gave a course called "Anthropology in Modern Life" over a period of twenty years, and it had the same title, but it was completely different; I changed the course about 15 or 20 percent every year.

"Traditional Cultures" was my first course as of that year, 1961, when the book [*Traditional Cultures and the Impact of Technological Change*, published in 1962] went to press. After it came out I had to change my lectures more than 15 or 20 percent every year. Then "Applied Anthropology" was the course I was giving in 1968, so the title [*Applied Anthropology*, published in 1969] is not so meaningful.

Riess: How were the graduate courses run?

Foster: Kroeber or Lowie talked for the first period or two. Then we might not meet for two or three weeks. Then we'd begin delivering our own seminar papers, so that we'd meet about ten times in the course of the semester. Perhaps the professor would talk the first three times; then we wouldn't meet for three or four weeks; then each student would have a whole period to himself to read his paper, and we'd talk about it and tear it apart, put it back together again. That's the way the seminars were.

Riess: Did you feel at all handicapped at Berkeley because of the gaps in your undergraduate education?

Foster: Well, I had a blow when I came to Cal, when I met Heizer and others and realized how much some people knew and how little I knew. Herskovits had essay examinations, on which I did pretty well. Kroeber and Lowie had fact examinations--I've got some of them here if you want to look at them--in which I had to remember hundreds of facts. I remember at the end of my first fall quarter of Lowie's world ethnology course I got a C in the midterm. That kind of rocked me back, and I realized I had to do another type of learning, rote learning, and I pulled my grades up after that, but it was a different approach.

You can criticize it, but in some ways I think it was awfully good because by the time we were done, all of us--whether we were archaeologists or folklorists or whatever--had a pretty good idea of the various kinds of societies around the world. And that certainly was good for us because, in my view, anthropology is, and should continue to be, a comparative discipline. It was very important to be able to put anything we learned ourselves against this wide background.

I had an example of this much later. Michael Maccoby, a social psychologist who took his degree at Harvard, worked with Erich Fromm. I met him in Cuernavaca in 1964 when we were first on sabbatical there. He came out to Tzintzuntzan once, and he noted something that I had not observed. He heard whistles and he asked about them. It's not a whistle language, but some names have whistles that--you can go by a fellow's house and whistle, and he'll recognize his name in your whistle.

Michael thought it was interesting just in itself. I immediately--when I find something like that--begin to think "whistle language." I think of the Canary Islands where they have whistle language. I think of some place in northern Mexico where they have whistle language. It's just a difference in the way our training prepares us to deal with new data. I find it very

interesting that anthropologists are always interested in the minutia of data. Clyde Kluckhohn at Harvard called anthropology "the science of leftovers," which I think is a very good term-- little bits of data that don't fit in anyplace until an anthropologist comes along and fits a lot of them together.

Riess: Then rote learning really worked. You remember.

Foster: I remember enough of it so that I think it was a good training. Let me see, I've got some exams here [going through papers] that I can show you.

Riess: You've kept all of your exam books?

Foster: A lot of them. This was the "First Mid-Term Examination" in Anthropology 102 [February 19, 1936]. Our text there was Darryl Forde's *Habitat, Economy, and Society*. Lowie gave the course.

Riess: "Chapters in Culture History," 102.

Foster: Yes, that's it. Here's the way the exam goes. "The natives of New Guinea (lack tobacco; have raised tobacco from a wild species of *Nicotiana*; grow tobacco identified as *Nicotiana tabacum*)." We were supposed to underline the right answer. "This fact indicates direct (pre-Columbian, post-Columbian)"--post-Columbian was the answer--"contacts of New Guinea with (Australia, Indonesia, India, South America, or Central America)." Indonesia is the right answer.

[Next question]: "The following plants were common to America and some part of the outside world before Columbus: (cacao, coca; sweet-potato; *Lagenaria* gourd; squash; maize; lima bean; potato)." And the answer is the sweet-potato and the gourd.

[Next question]: "Eduard Hahn was right in (describing all plough-less farming as hoe-farming; separating hoe-farming from plough farming; correlating the plough with masculine labor; correlating all primitive hoeing with women; emphasizing the non-practical aspects of early stock-breeding; denying that all farmers had once been pastoral nomads)." Well, that's a sample.

Riess: You really had to have the facts. You couldn't just have fuzzy impressions.

After each semester, did you bind your notes?

Foster: I kept them in these binders.

Here's the "First Mid-Term Examination," September 30, 1935. This is the one that really rocked me back and gave me a C. I missed twelve answers. "The modern Reindeer Chukchi use skin tents as dwellings, but anciently their dwellings were reindeer skin with a framework of poles." Whalebone was the answer.

[Next question]: "The Mentawai culture differs economically from that of the Ifugao in lacking rice and weaving." I got that right.

[Next question]: "The Bear festival is practiced by the Ainu and I said the Tungus, and I should have said the Giliak. "It consists in fattening a bear and ceremonially killing the bear, which is then expected to bring many bears back. The Koryak Whale Festival is similar in that the whale is expected to tell his brothers how well the Koryak treated him." And that's the right answer.

[Next question]: "Reindeer are (hunted, eaten, driven, milked, ridden, stabbed, guarded with dogs) by the Chukchi." I said eaten, driven and stabbed. I was right, but I forgot to put hunted, so I lost a point there.

Riess: In this article you wrote on graduate study you offer a list of the kinds of things you had to know.²

Foster: [reading] "The factual nature of learning is fully illustrated by upper division course examinations. In reviewing those I took I'm struck with the preponderance of questions dealing with the presence or absence of traits, their distributions, and the time sequences of their appearance. Examinations always were 'objective,' with words or phrases to be underlined or inserted in blank spaces in sentences. In one question, on the Indians of California, Kroeber asked, 'State the occurrence of the following thirty-eight traits in terms of these five California areas: Colorado River, Southern, Central, Northeast, and Northwest.' Among the thirty-eight traits were basketry cap, hairnet, sandal, sweat house lacking, plank-built canoe, gourd or turtle-shell rattle, iris fibre string, women's clan names, no marriage of widow by husband's brother, no scalps taken, and pit roasting of adolescent girl."

There must have been something good about this method, because all of us had the same kind of training, and yet it produced people like Cora DuBois, one of the first persons in

²"Graduate Study at Berkeley, 1935-1941," in *Essays in Honor of Walter Goldschmidt, Anthropology UCLA*, Vol. 8, Nos. 1-2, 1969.

culture and personality; Heizer, one of the best archaeologists that ever lived; Katharine Luomala, a famous folklorist; Margaret Lantis, an Alaskan specialist; Wally Goldschmidt, who was sociological in his outlook more than any of us; Julian Steward, who was distinguished as an ecological anthropologist; Duncan Strong, who was a well-known archaeologist. Something about the training made it possible for us to do well in any field we picked. I must admit I've often wondered why.

Museum, Library, Teaching Assistants

Riess: Tell me about how you used the museum, and the library.

Foster: Anthropology graduate students had access, keys, to a seminar room on the third floor of the main library, where the Bureau of American Ethnology volumes and the University of California Publications in American Archaeology and Ethnology, and a few other basic works were shelved. We could go and study there anytime and use those works, other than when Kroeber and Lowie were holding their seminars there.

The museum was open only two weeks in the fall and two weeks in the spring, and it had only a couple of small exhibition rooms. It was in the old engineering building, in the complex where Campbell Hall now stands. Museum specimens to illustrate the material culture of the peoples we were studying were laid out on glass-topped tables, or hung on a backing of burlap in vertical cases. Every semester the person in charge of the museum had to ask the teachers whether they wanted materials on exhibit. The museum was not an important part of my training at all.

Riess: Have you made it part of your teaching, have you used it more?

Foster: Not a great deal for the courses in general. If I were talking about material culture in a seminar, I would take students to the museum myself and pull out the trays and let them handle the materials. I think that was better than just hanging up specimens.

Riess: You talked about teaching assistantships. Were you a T.A.?

Foster: Yes, I was a T.A. for Ronald Olson in the fall of 1937. We got six hundred dollars--three hundred dollars for each semester--for the privilege of meeting six classes of twenty-five students each once a week and giving them exams and filing their grades. It was a wonderful experience. We learned how to teach.

Riess: Was Olson a good mentor, teacher?

Foster: He was a good example, but no, nobody ever taught us formally. Our fellow graduate students from time to time commented on their teaching experience, but really we were pretty much self-taught.

We were a homogeneous group. I think that's one of the differences as compared to today. Whether we were teaching assistants or not, we were in the teaching assistants' bullpen, as it was called. In that article we were looking at, "Graduate Study at Berkeley: 1935-1941," I tell about Wally Goldschmidt and Frank Essene and me writing our first paper on warfare between the two tribes in northern California we had studied. And it was Margaret Lantis, who was a couple of years older than me, rather than Kroeber or Lowie, who coached us on how to write the article and how to send it off in journal form.

I was astonished when I came back in 1953 to find that students were reluctant not only to discuss their ideas with each other, but even with their professors, for fear the professor would steal their idea.

Riess: You characterize it as homogeneous, but also it was non-competitive.

Foster: Well, non-competitive, certainly non-competitive, but also homogeneous in that we'd all had the same training. We didn't have to tell people what so-and-so had written. We'd read the same things because we'd had the same classes.

First Trip to Mexico

Riess: When do you think you began to get an idea of what aspect of anthropology interested you?

Foster: Christmas of 1936 I had a five-week vacation, and I didn't want to go back to Ottumwa for five weeks, so I decided I'd take a trip to Mexico. I went down by train. I left Berkeley Saturday evening, and traveling by train continuously, by Wednesday noon I arrived in Mexico City. You just can't believe how far it was. Wonderful trip. I went to Oaxaca and Veracruz and Taxco.

Riess: You were by yourself?

Foster: By myself, yes. And I came back up the west coast through Guadalajara, all by train.

After I got back, I went in to see Kroeber. I said, "Dr. Kroeber, I'm going to specialize in Mexico." He kind of discouraged me. I don't know whether he thought I wasn't bright enough or what. He said I would have to learn a lot of specialized history, etc., etc. But I kept at it.

Riess: Did you do a journal of that trip?

Foster: No, and I'm glad you asked that question. That's a great regret I have, that I never kept a formal journal. The closest I had to a journal are the batches of letters that I'd write to my parents, a lot of which were saved, but not all. Nobody ever told me to keep a journal. It's something we never were told by either Kroeber or Lowie or Herskovits, and I didn't tell my students to keep a journal until very late in the game. I regret it.

Riess: Did you set out on that trip with a Spanish-English/English-Spanish dictionary?

Foster: Not on that trip. Enough people in Mexico spoke English that I had no problems. When I got back, I realized I was going to have to learn Spanish, but I didn't begin until the fall of '37, that fall following my trip there.

Sent to Round Valley by Kroeber, and Tested

Riess: Then the next summer you had field research in the Round Valley, a sample of another direction you could have taken, I suppose, the California Indian.

Foster: I knew I was never going to do that for my life's work.

Riess: Why was that so clear?

Foster: Well, it just wasn't interesting. I'm glad I had the experiences, it was very useful. But I remember when--Kroeber in those days would send all of his students in the department to the field for two or three summers, to gather data. At that time he was interested in a culture trait collection that many of the students had to gather. And they just had to ask a series of questions: "In the old days, did you have this, that and the other?" Sort of like the questions I was reading.

When he sent me to the field, he said, "I want you to do an ethnography of the Yuki Indians in Covelo, Round Valley." I said, "Fine. But I don't know how to do fieldwork. What do I do?" He

said, "Well, you'd better buy a notebook and a pencil, I'd suggest." That was all the training we had. Wally Goldschmidt says Kroeber told him the same thing, too.

Riess: This [showing interviewer's notepad] kind of notebook? Stenopad?

Foster: Yes, yes, exactly, that was it.

Well, I went up. And he had said, "There is a good informant named Ralph Moore, who worked with me. I suggest you get in touch with him." And so I looked for Ralph Moore and found him. I introduced myself and arranged to pay twenty-five cents an hour for three hours in the morning and three hours in the afternoon.

After about the third day, Moore said, "I don't know why you're asking me all these questions. Professor Kroeber has it all written down in *The Handbook of California Indians*, and I have to read it every night before you come." I said, "Mr. Moore, I'll give you ten dollars for that book." He said, "Sold," and that's how I got my copy of *The Handbook of California Indians*, which I subsequently gave to Bill Simmons--who has now gone to Brown as vice president.

But I remember, after about a week I was so discouraged I came back and saw Kroeber and said, "Dr. Kroeber, I just can't do it. I can't be an anthropologist. I just can't do it."

Kroeber was very good. He said, "Well, you know, this is just a summer practice, it doesn't make much difference whether you get anything or not. Go back and try it again." I came back a second time and said, "Dr. Kroeber, I just can't do it." He said, "Well, I want you to go back and finish the summer." So I went back and found another informant, and at the end of the summer, to my surprise, I realized I had acquired a good deal of excellent data.

Ralph Moore turned out to be a less good informant than I had expected, but then I found a man named Eben Tillotson, who was a wonderful informant. Kroeber subsequently met him and said, "He's a fine informant." That gave me a lot of confidence. I got along very well with him [Tillotson] on a personal basis. We liked each other. He appreciated what I was trying to do. I remember when he would give me a term, he'd look in my notebook and see that I had written it correctly. He was literate.

He said, "I want you to make sure you get this right because my children and grandchildren are going to know about this only if they read what you write." I thought he had a very far-reaching

view of the role of the anthropologist. We were very fond of each other. I think of him with great affection to this day.

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Foster: That fall I wrote up my data in essentially the form that it appears in the Archaeological Records series. I showed it to Kroeber with a good deal of pride, and he read it carefully. He said, "You've got excellent data here. I'm very pleased with your data. Now, I want you to take it back and rewrite it and say everything you say here but use only two-thirds as many words." And that kind of rocked me back. He said, "You don't have to say 'the hat of the man'; you can say 'the man's hat.' There are all kinds of ways you can save words."

That was the first time I ever sat down in his office with him for any time, I think. It was a lesson that I took to heart, and I have tried to pass it on to my students. I think I had quite a reputation when I was teaching of demanding clear writing in their dissertations. I tried to teach them that you don't just tie two clauses together with an "and;" you try to build some contrast, "although," "but" or something like that.

Riess: That's a good story. Saying, "I don't think I can do this" and having Kroeber send you back.

Foster: That's really the reason I started taking students to the field myself in Mexico. But there was another reason. We had a very bright anthropologist, a young student, about 1955, after I returned to Berkeley to teach. She went to the Pacific to do a tough job of research and dropped out of anthropology. I talked with her later and said, "It's such a shame. Every anthropologist has the experiences of doubt and worry that you had." She said, "I wish you had told me that before I went to the Pacific."

I thought, Oh my God, we've got to do something with students. I started taking students to Mexico in 1959. I think that was the first formal training any of the ethnologists had in a foreign country, certainly, with any of our faculty members. I think it has been very good.

Riess: And your experiences of doubt and worry?

Foster: I don't remember why I was so despondent. I was lonely, and it was a very different experience for me.

Riess: I mean, is it a kind of culture shock?

Foster: Yes, it is culture shock. It's hard to go out and meet strangers and say, "I want to ask you a lot of questions, some of which will be pretty intimate, about your life." "Establishing rapport" is the catch phrase we use in anthropology. If you can't establish rapport with informants, you might as well give up the ship. To succeed you've got to establish a friendly working relationship with people with very different backgrounds and customs and beliefs.

I've always been quite a reserved person, and it's not easy for me. I knew that Ralph Moore, having worked with Kroeber, had him in mind as the ideal anthropologist. He probably thought, "What's this young whippersnapper doing asking me these questions, making me read up every night?" But it was when I realized I could strike a real friendship with Eben Tillotson that I first developed the confidence, and I've done well in the field ever since.

Riess: It's a different confidence than the confidence that you exhibited by getting on a train and going off to Mexico for five weeks.

Foster: Yes. I could always come home then if I wanted to. I didn't have to establish contact with anyone if I didn't want to. I think culture shock comes from the fact that people who go out and have to live and work with people are in a very different position from tourists who go to the same country. When you go out and have to work with people, you've got a goal, you've got to produce something. You spend three or four months, and you realize you're not getting very much, and you think you're a failure. Tourists can always come home, but when you realize you're being tested, it's quite different. I've seen a lot of technical aid specialists who crash, and a few anthropologists, too.

Riess: You say you realize you're being tested. The community is testing you?

Foster: You're testing yourself. When you go to the field, you've taken money from a granting agency; you've told them what you're going to do, what you're going to prepare; and if you don't get the data, you've failed. Then you're going to feel badly all your life. I mean, you know what's expected of you, but you're not sure you can do it. That's what I mean by testing.

Riess: It's not that in the encounters with the people you're constantly being tested by them.

Foster: Well, you're being tested by them, too. In my doctoral research I'd planned to work in a village other than Soteapan, where Mickie and I ended up. But after about three days we realized we'd



George Foster and Mary LeCron, Northwestern campus, 1935.

better get out of that village, we weren't going to get anywhere. We might not even come out at all if we didn't get out, and quickly!

Meeting and Marrying Mary LeCron

Riess: I'd like you to describe that. But first, I think you should introduce Mickie into the story. At the end of the last tape you said she was one of the attractions in that Herskovits class.

Foster: At Northwestern I went with several girls, none very seriously. "Dating" is the word we used in those days. It was quite different from what it is today, the relationship between the sexes. The spring of '34, when I took this class, Mickie was in the class, and also her close friend, Julia Tanner--Julia, who married the twin brother of Mickie's brother-in-law, Pete Jurs, ultimately.

I saw Mickie, but she didn't pay any attention to me. I didn't ever speak to her until the following fall, when Herskovits--we declared ourselves as majors then--took all the majors, of whom there were five, to a meeting of the Explorers Club in Chicago. She kind of set me back when she expressed surprise that I had been in the class the semester before, but we certainly fell in love. My last year at Northwestern was made much more exciting by the fact that we were going together.

Then that summer Herskovits sent me on an archaeological dig with Wil [W.C.] McKern, who was director of the Milwaukee Public Museum, and a Berkeley Ph.D., incidentally. Or no, I don't think he was a Berkeley Ph.D. Sam [S.A.] Barrett, his predecessor, had been a Berkeley Ph.D. Wil McKern was a very, very, very nice fellow. We were digging a mound up near Spooner, Wisconsin, and Mickie and I wrote back and forth a good deal.

Then I went out to Berkeley and got started. For various reasons we drifted apart. We didn't get back together again until the fall of '37, when she came out to visit her sister, Florence Jurs, who lived in Oakland. And she decided to stay on and audit courses.

At that time, I was living with Dimitri Shimkin in Lowie's house on Benvenue Avenue. Lowie had gone to Yale to teach, and he asked Dimitri and me if we'd like to rent his house. He gave us a good price. And he said, "There's a young man named Carlos Garcia, a Mexican student, who knows how to cook, and he'll clean the house." So I was living in Lowie's house then. And Eleanor

Yonge, a major in the department who also lived on Benvenue, a block south of Lowie's house, she started going with Carlos Garcia and ultimately they married.

Then Mickie came out, and we took up where we had left off, and before the end of the semester we had planned to get married. She had spent a good deal of time in Europe with her folks. Her senior year in high school and her first year in college had been at Grenoble, and some time in Vienna. I was anxious to see Europe, so we decided we'd take the whole year off, 1938, and we were married on the 6th of January, 1938, in Washington, where her father worked in the Department of Agriculture at the time.

Time Out: Austria with Mickie, 1938, the Anschluss

Foster: We sailed to Europe on the Cunard liner *Scythia*, and we spent several days in Liverpool with some of my distant cousins. Then we went to London. Jack Trevor was there, the fellow I told you about at Northwestern? I remember I was amused at his question. He said, "Are you and Mickie married, or are you just traveling together?" The idea of traveling with a woman and not being married to her was so foreign to me that--I couldn't believe the question. [laughter]

I remember one of the highlights was attending one of Malinowski's seminars at the London School of Economics. That experience opened our eyes because there were black students from Africa, and students that had been in the field for several years. I knew I was in the big time then, compared to anything I had had at Berkeley. Then we went on to Geneva. We were headed toward Vienna to study German.

First we found a place to live--it was a house with an elderly woman and her unmarried daughter on the Ringstrasse. We took one meal a day, breakfast and one meal, our choice, either noon or evening, and we ate the other meal at a cafe or a restaurant in the city. So we got to know Vienna very well. We took our German lessons in the morning, and then we worked in the afternoon. Then we'd go out and sit in the cafe and read a newspaper and watch the world go by.

Riess: Were you interested in speaking German, or were you after a more scientific German?

Foster: I was anxious to learn to speak it, but I was never able to--well, I could speak simple German--I was interested both in improving my

reading knowledge and acquiring enough speaking knowledge so I could get along, which I did for a while, but it quickly left my mind.

Riess: You got to Vienna just as Hitler was preparing to take over.

Foster: The *Anschluss*.

Riess: Was that just in the background? Or were you so aware of it that it was uncomfortable?

Foster: We had no idea that what was going to come would come as dramatically and as rapidly as it did, but we knew from the time we got to Vienna that the Austrians were very discontented with their Socialist government. The picture of Austria as a poor little nation raped by Hitler, as portrayed in this country, was not according to the facts as we saw them. Hitler was welcomed by a majority of the Viennese when he came in. They thought he was their savior--clearly not the case. It angered us to read *Time* and *Newsweek* and find that the account that was given to the American public was so biased, so different from the one we had.

We'd go to a movie in the evening and the premier, Kurt von Schuschnigg, would speak on film about the greatness of the Austrian empire and that Austrians must do this, that and the other thing. The people would sit in stone-cold silence. It was clear that he was not in the least popular.

Then the *Anschluss*. There was a lot of preliminary talk, and meetings. There were soldiers marching in the streets. I forget the sequence. One day when the soldiers came, there were hundreds of airplanes flying over the city. I'm sure many of them were the same ones going back and forth, but it was a show of force that I had never seen anywhere, and it convinced me that Hitler was a man to be feared. He had the means to do anything he wanted in Europe.

Hitler came that same day, or the day after, and was there for several days. He was driven along the streets, standing up in the open. Anyone with a pistol could have just popped him off. Then I remember a vast--there must have been a hundred thousand people in the Hofburg garden when he spoke. And Hermann Goering and [Joseph] Goebbels were all there. Mickie wrote some letters to her folks which were published in the *Des Moines Register and Tribune*, her first published works, which are kind of interesting, telling about our experiences. We stayed there until well after the *Anschluss*. It was June before we left.

One of the pleasant things about our stay there was becoming acquainted with a young Austrian anthropologist named Christoph von Führer-Haimendorf and his English wife, Betty. He worked in the museum and he also taught anthropology in the main university. Later he went to the London School of Economics and became famous for his research on the hill tribes of northeast India.

That was an interesting time to be in Vienna. There were still a good many American doctors who went to Vienna to polish themselves off and get a bit of patina on their degrees, so that every time Mickie and I would go to a cafe, it was always assumed that I was a doctor; I was always addressed as "Herr Doktor."

A Summer Traveling

Foster: Then we decided that we'd like to spend the summer traveling before going to Paris to study French. Mickie's father sent a Chevy sedan over by the Arnold Bernstein Line. It cost a hundred dollars in those days to ship a car from the United States to Europe, and the European cars seemed so unsatisfactory that it was less expensive and wiser for us to have a new car shipped to us. The car came into Rotterdam, and after we picked it up we traveled through Scandinavia where we were joined by Betty and Christoph.

We went up through Norway and across into Sweden, where, for the first time, I drove on the left side of the road. Then we came down to Stockholm, and crossed to Copenhagen, where the International Congress of Anthropological and Ethnographical Sciences was having its second or third meeting. I forget which. We met a number of anthropologists. Herskovits was there, and he introduced us to such people as Marcel Mauss and Kaj Birket-Smith.

Malinowski also was there. He remembered us from our attendance in his seminar, which was flattering. We had actually met him first of all at the Herskovitses' in Evanston. Since Herskovits had no graduate students--he had only five majors when we were there--he treated us as graduate students, and when somebody such as Malinowski or Katherine Dunham would come to town, he'd invite us to his home. A wonderful experience.

Riess: Katherine Dunham, the dancer? Why would she have been there?

Foster: She was an anthropologist also, and Herskovits was interested in her dancing--evidence of his aesthetic sense of black music and black art.

Riess: With a couple of years of anthropology under your belt, did you look at people as you traveled in a different way, do you think?

Foster: Yes, I did. I was aware of cultural difference very much then. Also I began--my ideas on envy, I think, began there when I realized that the word for "tip" in German is *Trinkgeld* and the word for "tip" in French is *pourboire* and then a tip in English clearly comes from the word "tipple." I didn't do anything with those ideas for another thirty years, but they were basic in the work that I did on the anatomy of envy.³

Later, I gathered all the words I could for tip and found they all had to do with inviting people to drink with you, and that convinced me that "tip" is not an acronym for "to insure promptness," which is the popular explanation. However, I don't know how "tipple" came to mean tip in English.

So yes, I was aware of cultural difference. I didn't take notes, but I made mental notes that stood me in good stead.

Mickie's Career Plans

Riess: Back to Mickie. You said she was a year behind you at Northwestern and then went to Columbia.

Foster: Yes, where she had a full year of graduate work, so she had had as much anthropology as I, since we began studying at the same time. She had two undergraduate years and a graduate year, and I had one undergraduate year and two graduate years.

Riess: How did the two of you talk about having two careers?

Foster: Well, she didn't think about going on with anthropology then. Jeremy, our son, was born in March of 1939. We were married in 1938. And she just expected to be a mother. Melissa came along three years later. In retrospect, Mickie was very frustrated because she didn't have enough to keep her mind busy. She has a very active mind, as you can tell. She was the one that got interested in psychiatry and found out about the course given by Karen Horney.

³"The Anatomy of Envy: A Study in Symbolic Behavior," 1972, *Current Anthropology* 13:165-186, and "Reply," 198-202.

It was not until we came to Berkeley, after we had been here a couple of years, after Melissa--no, it was before Melissa finished high school, but not very much--that she decided to go back to school. It must have been about '58. I was chair of the department then, and it seemed inappropriate for her to study anthropology where she'd be judged by my colleagues. She by then had developed an interest in linguistics, so she registered in the linguistics department and took her degree in linguistics. Earlier, during my doctoral research and writing, she was very helpful. She gathered a lot of the material I used.

When she began working on her own I was kind of pushed back and had to do all my own work. She would read my papers, though, and was very helpful. She was very busy herself. There has never been any question of division of work, really. She's the linguist, I'm the ethnologist.

Riess: And there was not a conflict in the beginning.

Foster: I would not have objected if she wanted to go into anthropology, but it just didn't cross my mind. In retrospect, it's weird that it didn't.

Riess: Weird that it didn't cross your mind or that you didn't have a discussion of it?

Foster: We both assumed that now her role was mother. That she had a good background in anthropology and would make a good helper to me. But I find it strange, in retrospect, that we didn't realize that she needed the stimulation of graduate study.

III MEXICO, 1940-1941, AND FINISHING GRADUATE SCHOOL

[Interview 3: November 24, 1998] ##

On Taping and Note-taking in the Field

Foster: My first experience with mechanical recording came shortly after we returned to Washington from Mexico, about 1947. We bought a primitive recorder; it was a box with mahogany veneer about eighteen inches square and a foot high. As I recall it had a big reel, seven or eight inches in diameter. Sound was recorded on wire rather than tape, and there was no way to splice the wire that I knew of. I didn't use it for interviewing, and it was not designed to be taken to the field. We used it to transcribe music from phonograph records and things like that. We could record our voices on it.

I remember we had a wonderful time fooling some friends. Before they arrived we turned on the recorder and then started out by saying, "You don't think of us as a musical family, but while we were living in Mexico we learned to play the guitar and sing Mexican songs." We followed this by transcribing several Mexican records, and the transition was so smooth it was amazing. When our friends heard the recording they were astonished at how well we played and sang!

Riess: Are those recordings stored somewhere?

Foster: Oh, no, we never kept them. They were mostly just Mexican records we transcribed on wire, just for the fun of it. We'd put them on and they'd run for an hour or so, or half an hour or so, in the days when records were all on 78s.

When I was in the field in 1937 for the first time, it never occurred to me to record. The first recorder I remember that I had in the field was something called a "Midge-Tape," about 1960. It was about nine inches high, three inches wide, and an inch-and-a-half deep. It had so little power from the battery that one had

to rewind the tape by hand. The tape was used only for recording. But that was not satisfactory, and it was not until these cassette recorders came out, like the Sony I showed you--that's the only one I've used extensively, since the early 1970s. I have many tapes from that, which I have had transcribed.

I think anthropologists make a mistake in recording so much by tape. The tape is useful, of course, when you want the exact wording. For dreams, for example, it's wonderful. And for accounts of illness I found it very useful. But if you're using it as a substitute for taking notes you're making a bad mistake, because in taking notes one automatically filters out 90 percent of the crud that comes along anyway. You can't write it that fast.

When you're recording with a tape, you get lazy. You think, "Well, it's on the tape, I don't have to follow quite as intently." I found I would miss a lot of things that I would have followed up if I had been doing it the old-fashioned way, just listening and making sure I understood each point and had it down clearly before going on to the next. [See further discussion, p. 240.]

Riess: What kind of shorthand did you have, then, for taking notes?

Foster: I had no shorthand. I either wrote slowly when I had an informant who was willing to go slowly, or I would just jot down something, a word or two to jog my memory, and then I would go back to my typewriter and type it out in full.

Riess: Do you think that duplicates the experience that you get in college, of taking professors' notes, or is it a different kind of filtering?

Foster: Filtering is filtering. Most of what you hear in conversation is not critical to the development of an idea or a theme. If you had to listen to it on the tape, if you had to re-listen to a lecture, it could drive you nuts. You can get the meat in a relatively few paragraphs. That's much more efficient, I find.

I was violently opposed to recorders for a long, long time. But then, when I began recording dreams, and when I wanted to get detailed information on medical beliefs, I found it very useful because I picked up words that I hadn't heard because I didn't know them. When I'd be listening to Spanish, I'd get a word that I'd think I understood, but it was not critical, it seemed to me, so I never registered it. I found that I had a lot of valuable material on the tape when I played the whole tape over.

Riess: Somebody would transcribe this for you?

Foster: Yes. I haven't done much transcribing. I've had Spanish-speaking people who could transcribe.

Riess: Your informants--were they bothered by having the tape recorder or having you take notes?

Foster: In Tzintzuntzan, by the time I had a tape recorder, everybody knew I was asking questions, and I would carry it along with me when I'd go to their houses, and I would say, "I'm going to turn on the recorder." They'd say, "Fine." Or if they came to my room I'd put it on the table and turn it on; they could see it all the time. But they didn't think about it.

In taking notes, when you listen--well, this is another topic. I don't know if we want to get on this or not.

Riess: It seems to hang together.

Foster: Well, in taking notes, I noticed when I take students to Tzintzuntzan, which I've done, most recently this last summer, they're anxious to get started recording data. I'll take them to the house of friends, and they'll pull out the notebook and start right away. And I've had to say, "Now look, you can get away with this when I'm here because they know me, but they don't know you. I would urge you not to do this with anyone until you establish good rapport on your own."

I'd say, "Try to remember what's being said. If you have to write something, ask how the word is spelled so you've got a bit of a cover." After you establish rapport, then, I've found, people don't care at all whether you're taking notes out in the open. I've often had whole lists that I go through, asking "yes or no," or "hot or cold," or "wet or dry." It's part of the technique of learning to interview.

On the Good Informant

Riess: Do you find that if you stop and ask someone to go into something in more detail that they tell a remarkably enriched and different story?

Foster: It depends on the informant. Some do, and some don't. I don't think you can generalize. Generally, I find only about 5 percent of the people in the community are really good informants. We tend to think that everyone can tell us something that's interesting, and a lot of people can, but I think every

anthropologist finds that the kernel of the data that he or she has comes from relatively few people.

For example, we found this when we were working with the Popoloca in 1941. We had a couple of men that we used a good deal as informants. One was the schoolteacher, who had no students because the school burned down, but he was there, collecting his salary. And the other was named Leandro Pérez, who was just a farmer. He was one of the best informants the world has ever produced.

When we were working on linguistics, for example, he quickly got the idea of what we wanted when we asked, "How do you say 'I go, you go, he goes, we go, you go, they go?'" Most informants when asked, "How do you say 'I go'?" reply with 'You go,' but he got the idea quickly. After about the first hour, we didn't even have to say, "Give us the whole series," the whole paradigm. I'd say, "How do you say, 'I sleep'?" He'd reply, "I sleep," and then he'd follow with, "You sleep, he sleeps, we sleep, you sleep, they sleep," the six forms. And that's the mark of a good informant. A good informant knows what you want and gives you what you want. With poor informants, who are most of the people, it's just like pulling teeth to get good data from them.

Riess: Do informants think about it between sessions, do you think?

Foster: Well, if you're working steadily with an informant, yes, they do. And linguists work particularly with the same informant, so yes, they catch on very quickly.

Riess: It's an interesting idea, to become an informant in your own community. What a context in which to view yourself and your friends. It must be a very changing experience for the informant.

Foster: How do you mean that?

Riess: Whereas they are naive participants in their society up to that point, they then become sophisticated participants, participant observers.

Foster: I suppose they do. I've never really thought about it like that. I never thought to go back and ask the informant if they see their society differently than before I began asking them questions. That's something I'm going to ask this Christmas, when I go down to Mexico, see if they can give me an answer. I doubt that they'll be able to give an answer, but they may. The family with whom we live--we've lived in their house now for forty years--they've been here on six different occasions in Berkeley, and I'll ask them. They'll give interesting answers. They're very bright.

On Discoveries, Big Ideas

Riess: The last time we met we ended with your talking about tipping, and the paper about envy. I understand, from having read the paper about envy, how tipping works.

Foster: An envy-reducing mechanism.

Riess: My question is where do you register such pieces of information? Did you actually write it down?

Foster: No, no. I just kept it in my head. I didn't realize at the time that it was valuable information.

Like the Mexican custom that triggered [for me] twenty years later the idea of social distance. When you meet somebody on the trail in Mexico, as you draw closer, you say goodbye to each other. That's something that puzzled me for a long time, but I didn't write it down anywhere, I just remembered it. I don't know how many things I've lost because I failed to write them down. Maybe there are all kinds of jewels there that I've passed by. I don't know. [laughs]

Riess: When we first met you said you'd be interested in exploring, in the oral history, the serendipitous nature of discovery in anthropology.

Foster: And in any other field as well. I've been interested in my reading by the fact that I've encountered very few statements as to how scientists came by their big ideas. Walter Alvarez, in his wonderfully titled book, *T. Rex and the Crater of Doom*, is the best thing I've encountered in book form. There he describes the whole process whereby he came to the conclusion that it was a tremendous meteor striking the Yucatan Peninsula sixty-three million years ago that resulted in the extinction of dinosaurs and a lot of other forms of life. It's a wonderful account of how different hypotheses arise. He [Alvarez] would test hypotheses against his own observations, his own ideas, discuss them with colleagues, and ultimately come to a conclusion--and I think he does a very good job in the book of describing the process.

Unlike science in laboratories, where you can duplicate experiments, in anthropology for the most part you can just say there's consensus. Consensus is about as close as we have to proof. It's like evolution, Darwin's work. It never could have been reproduced, but the consensus of opinion is that he was on the right track and did a remarkable job.

Riess: In your paper on envy you acknowledged in your "Reply" the problem in trying to create all-inclusive theories.¹ Do you deliberately not tackle precipitously what seems to be a promising idea?

Foster: I don't go to the field and test hypotheses that I dream up at home. My best ideas have come from chance observations in the field. When I see something that strikes me as being odd, not what I would expect, then I begin to cogitate, and if I'm lucky I'll read something or something will occur that will bring it all into focus.

The way I got started on envy in Tzintzuntzan was this: when a group of children came to have their school breakfast at Micaela's house they ate in absolute silence--and that's not something an American expects of primary-school kids. You'd expect bedlam, and you'd have bedlam. I was puzzled as to why they didn't make noise. When I asked Micaela, she said, "They're taught not to talk when they're eating." I said, "How do you teach them?" She said, "We tell them the guardian angel is watching over your eating. Be quiet. Don't talk."

Then I began to wonder--there must be some value in not talking. And I remembered that when we were eating at the house, even though there were thick adobe walls--nobody in the street could possibly hear--whenever we'd get on a delicate subject, the voices would lower, and they would say, "We don't want anybody outside to hear what we're talking about." That struck me as odd.

Then I began to realize that food is a very valuable commodity in a peasant community because not everybody has as much food as they'd like to have. Some food, in the form of fruit or snacks, is always on the table, so if anyone comes you can offer them a snack. But when it gets down to the beans and tortillas that fill a stomach, you want to keep most of what you have for yourself.

I realized that that's one of the reasons for eating quietly. You don't attract people, they're not apt to drop in expecting a handout, if you're eating quietly. If you're having a raucous meal, they know there's food there. There's a lot more, but I won't go into it all now, such as the greeting on a postcard, "Having a wonderful time, wish you were here." Who would think that's got to do with envy, the subconscious fear of envy?

¹"The Anatomy of Envy," op cit.

And tipping. The thing that strikes me about tipping is we tip only people who have considerable power over our physical well-being or our possessions: porters in hotels and railway stations, barbers--razors going around my ear, I often think, "I hope he thinks well of me." And waiters, of course. I think tipping probably began when people began eating outside of their homes, where they weren't immediately preparing the food themselves.

That envy paper is one of my favorite articles. Margaret Clark, who was my first Ph.D. in medical anthropology, says she thinks it's the best thing I've ever done.

Riess: Would the church--do you think they imposed silence in eating in the schools?

Foster: No. The schools are all state or secular schools. There was a period during the first field work in '45 and '6 when there was a small church school, but that didn't last long and it was not a success, fortunately, in my view. And why would the church care whether you eat silently?

When I've eaten with a group of priests at Tzintzuntzan, as I have on many occasions, they enjoy drinking and telling stories, and they whoop it up. There's no silence there. They're not worried about people knowing they're eating and having a good time. No, I don't think the church has any influence on that. It's something you find in many parts of the world. Eskimos, for example, I think you find that in winter they eat inside, quietly; but when they're in their tents in the summer, they eat openly, and anyone who comes by gets fed.

The custom in Mexico and Spain when you're eating in a restaurant, where anyone can see, if anyone comes in you symbolically invite them to join you. "*¿Gusta usted?*" "Would you like to sit down and eat?" I think the points I make in that article hang together remarkably well.

On Psychology, Dream Work

Riess: Laura Nader, in the Kroeber Anthropological Papers, talks about your work [reading] "deepening the understanding of human behavior." That makes me think that you're more a psychologist than anthropologist, and it makes me want to ask you whether psychology had an appeal for you.

Foster: The only psychology course I had at Northwestern was given by the dumbest professor I ever had. The course came at one o'clock Monday, Wednesday and Friday, and I was sleepy anyway, from having a good lunch--not a good lunch, but a filling lunch--and he would say, "Now, on page 122 of my text, I say,"--then he'd read from his text. If there is any way of killing the interest in a subject, that's the way to do it. No, I never gave a thought to psychology. I have had very little psychology along the line.

Riess: And yet you've done the dream work and the T.A.T. work. Is this within the purview of anthropology?

Foster: I got started collecting dreams when a young woman told me this wonderful dream she had about how she dreamt she died, and when she came to she was in a beautiful field, flowers everywhere. And in her dream a strikingly beautiful young woman came and took her by the hand, and she recognized this young woman as the Virgin Mary. They walked through the fields, and they came to this vast cathedral, and inside were all the angels playing their guitars, *plinkety-pluck, plinkety-pluck*.

I thought--that started me on two things: one was the recognition that I could learn a lot about people from their dreams, if I had a collection. The second was the fact that I had never dreamed music. I began asking, and I found that very few people ever dream music. Do you dream music?

Riess: No, I think not.

Foster: Immediately after that, I began dreaming music myself. And periodically, I dream music. But I had never dreamed music before that, and I find most people don't. Just something like that triggers my interest.

Riess: When you entered that arena, then you probably read more in psychology.

Foster: I read a number of books on dreams, yes. I've not done a thorough job of analysis. I'm not qualified for that. I hope somebody will someday do more with the collection.

And much less [have I done an analysis] with the T.A.T.s. I've got a wonderful collection of T.A.T.s. When I was teaching the course on Mexico, to illustrate the T.A.T.s I had slides made of the ones that were most revealing to me, and I projected them in the class, and asked the students to fill them out, to write their little story. I was enchanted to find that--there's one in which a man is hanging on a rope and kind of swinging in space. In Tzintzuntzan, and among the Mexican Americans in this country,

almost always this individual is identified as a thief who is letting himself over a wall down into a patio on a rope. Among the Americans, he's a young man who is athletic and he's demonstrating his athletic skills. I find this absolutely fascinating. The students did, too.

Riess: How about Rorschachs?

Foster: I have not done Rorschachs, but when Michael Maccoby, a social psychologist who worked with Erich Fromm in Mexico, came to Tzintzuntzan to visit us once, we went around and he recorded a number of Rorschachs. We have one article in which we deal with the Rorschach responses of Micaela Gonzalez, a wonderful old lady, the mother of the family with whom we stay, as well as her dreams and her T.A.T.s. But that's the only experience I've ever had with the Rorschach. I'm not qualified as a psychologist at all.

I'll give you another example: this is one I'm still waiting to find out what it means. One of my Ph.D. students, Theron Nunez, worked in a village near Guadalajara called Cajititlán, on the edge of a lake, which has subsequently dried up. On one occasion when I visited him there he told me how one afternoon he was in the bar with some of the boys, drinking, and a dog wandered into the area of the street outside the bar. A fellow pulled out his pistol and shot the dog through its haunches so it was paralyzed in the hind legs--it sat there upright but it couldn't move.

As the afternoon wore on, the men would pick up a brick or a stone and throw it on the dog. Finally, the dog expired. After the dog expired, the three-year-old son of one of the men who had been engaged in this activity picked up a stone and went over and dropped it on the dead dog, and his father just gave him hell. I'm still puzzled as to what the meaning of that is.

I use that illustration often in teaching, as to how you'll see something, you know it's significant, but you can't for the life of you figure out why. I'm still hoping before I pass on to know what that meant, to be able to relate it to other behavior.

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Riess: In the scholarly world of anthropology could you publish a story like that and ask the readers of whatever the journal is, "Has anyone had an experience like this, or heard of anything like this? What does it mean?"

Foster: There are places you can do it, but it's not done very much. Usually, one asks for a simpler--"In your community, have you

found this, that, or the other form of behavior?" I'm sure there are accounts of examples where an anthropologist will repeat something like this, but I don't recall ever having seen it.

Back to Graduate School, 1939

Riess: Now, back on track, what next after your European tour?

Foster: Mickie and I got back in time to spend Christmas with my parents in Iowa, in Ottumwa, and afterwards we drove back to Berkeley, and we rented our first home, an unfurnished four-room house on Creston Road, overlooking Grizzly Peak, for fifty-five dollars a month, I remember. It was a cute little house. It had a fabulous view. Two bedrooms, a bath, a living-dining room, and a nice little kitchenette. We were very happy there for a year and a half.

I began reading, obviously, and I took seminars in all of 1939. I took my writtens in September of '39--we don't have writtens anymore. Thirty hours of writing, six hours a day for five days. The usual plan was a solid week, Monday through Friday. Kroeber and Lowie gave us a break by starting us on Wednesday and continuing through Friday. We had Saturday and Sunday to recoup, and took the last on Monday and Tuesday.

Bob Heizer and I wrote at the same time. I remember it was scorching hot. It was mid-September, one of those hot periods we get in the fall here. We were just about reduced to a pulp. I was surprised because Bob, who knew and knows--has always known much more than I do in anthropology--was failed on one of the questions, and he had to take it over a few months later. But that did not reflect on his record at all. Kroeber just wanted to make sure that we knew enough to legitimately pass.

So I got through. After that came my oral examination by a committee of five: Kroeber and Lowie, Carl Sauer, Frank Knight, an economist, and Herbert Bolton, the historian. That was more or less a formality. It was assumed that if you could get through those thirty hours of writing, you knew enough anthropology to be a Ph.D.

Riess: Were the thirty hours all essay questions?

Foster: That was the nature of the written exam: write on the age-area complex, write on totemism, things like that. Essay questions, or paragraph answer questions. They weren't true-false or anything

like that. But you had to know a lot of facts if you wanted to get through the exam.

The oral examination I recall with pleasure. Bolton was a joy, he wasn't going to flunk anybody, and he seemed to be enjoying it as much as I did. Frank Knight was very tolerant. He knew I knew nothing about economics, which was the topic I was going to do my dissertation on, but he was a gentleman, and I've always been grateful to him. Carl Sauer interviewed me on his interest in the origin of plants and domestic animals, which has always intrigued me ever since I studied with Sauer. And Kroeber and Lowie asked the usual kinds of questions, there were no surprises there. So the oral examination--when I went into it I knew I'd come out of it all right. I wasn't worried.

Mexico, Spring 1940, and New Contacts

Foster: Before beginning my doctoral research in Mexico, I knew I had to master Spanish. So I decided that the next year, 1940, I'd go down to Mexico for the whole spring and study Spanish intensively, and decide specifically where I was going to work. Mickie and I spent Christmas of 1939 with her folks in Washington, D.C., and almost immediately after Christmas I started driving to Mexico.

It took ten days, I remember. I left on the 3rd of January and got to Mexico City on the 13th. I wasn't pushing myself, but it wouldn't have been easy to have driven much faster. The roads, both in this country and in Mexico, were not up to anything like present-day standards.

Riess: Were you equipped to repair your car if anything broke down?

Foster: No, I've never been equipped. I could patch a tire. That's all I've ever learned to do.

Riess: Did you have any trepidation?

Foster: No, because the road was paved all the way through to Mexico City from Laredo. And there was only one road, so anyone who was traveling to Mexico had to go on that road, and there were a good many Americans going by, and other Mexicans who spoke good English. I wasn't worried.

I got to Mexico City, and I had two people that I looked up. One was a young woman named Irmgard Weitlaner, the only person I know who was born on the 9th of October of 1913, my birthday. She

is the daughter of an Austrian engineer named Roberto Weitlaner, who as a boy was fascinated by Indians. About 1914, just before the World War began, he came to New York City and within twenty-four hours he was on the Iroquois reservation in upstate New York, recording language! He later moved to California, and Irmgard was born in Los Angeles. No, he must have come before that. He must have come about 1911.

In any event, in 1936 Irmgard had come to Berkeley to study anthropology and she married a young man named Jean, J-e-a-n, Johnson, who tragically was killed during the war. By then her father had moved to Mexico City, where he lived nearer to Indians. He had decided the United States was hopeless as far as delving into real Indian culture was concerned, but he loved Mexico. For much of his life he had to work as an engineer in a steel company in Mexico City, but every weekend and every vacation, he'd be out in the field on trips. Jean and Irmgard were married by then, and they were spending the holidays in Mexico City. So I got there in mid-January of '40, and they were there.

I had another friend whom I had met on my first trip in 1936 to Mexico, named Al Gartside. I had met him through a schoolteacher named Wilma Bihler, who had been on the trip to China in 1933. I had kept in touch with her, I saw her from time to time in Berkeley when I came in 1935. She had gone to Mexico a couple of years before 1936, and she met this fellow, Al Gartside, whose father was English. I think his mother was English, too, but for some reason he was an American citizen--he had worked in a bank in Los Angeles for a number of years.

In any event, while in Mexico on this trip they found each other quite interesting, and she said--I talked to her in the summer of 1936, before I went to Mexico--she said, "I've got a good friend there. Look him up." I did look up Al. By then he was married to a very nice Mexican woman named Aida. We liked each other. He took me to my first bullfight, and to Xochimilco. We kept up our friendship for many years, until he died, fifteen or twenty years ago.

So I had two people to look up. I got in touch with Jean and Irmgard and her father right away, and also Al Gartside, whose older sister, also named Aida, was married to an Englishman. He was quite well to do, but in spite of that, she was happy to earn a little extra money. So she became my tutor in Spanish. She was not a natural tutor, but she had been born in Mexico and spoke the language fluently. And she was a wonder on *modismos*, idiomatic expressions, which are very important in the Spanish language. I learned a good deal from her.

About the middle of February Mickie came down to Mexico with my parents and my sister, Janet, who is thirteen years younger than I am--she was fourteen then. We spent two weeks driving around Mexico. That's when I first went to Pátzcuaro, in February of 1940, passing through Tzintzuntzan. I never dreamed I'd ever be there again, but I knew the area, so that was one reason I was comfortable going there in 1944.

Well, I studied Spanish, and Mickie stayed on. We found a family named Oria. He was a Spaniard. She was an upper-class Mexican who claimed descent from some of the viceroys. They had a Porfirio Diaz-type house, from about 1890, on the second floor of which lived Antonio Caso, Alfonso Caso's older brother, a famous Mexican philosopher. I met him only once, quite casually, I never talked with him. But he was as famous in philosophy as his younger brother, Alfonso, was in archaeology.

Sr. Oria was kind of amusing. He was quite scornful of everything Mexican, and particularly of Mexican archaeology. Explaining Teotihuacan, the Pyramid of the Sun, he said, "That's just deceitful, just something dreamed up to attract American tourists. That was just a hill, and they encased it in stone and called it a pyramid, but that's all it was, just a natural hill." He really believed this. [laughing]

They were useful in helping us learn Spanish. Also, a friend of ours named Enid Zacharias, whom we'd met--I became acquainted with her when she was at International House at Berkeley in 1935. We were friends for many years till she died a year or so ago. She came down to Mexico and lived in another room in the Oria's house, so the three of us were all there studying Spanish.

Riess: What does it mean, a Porfirio Diaz-type house?

Foster: Porfirio Diaz was the Indian dictator of Mexico from about 1875 to 1910. I don't know if it's a recognized type of architecture or not, I think it probably is. Just a two-story brick house with a steep, British-type slate roof, and stone sills--a solid, well-built house, probably some with towers and gables, European in style. I call it Porfirian because it was obviously built during that period.

Another person I met around this time was Isabel Kelly, who took her Ph.D. in anthropology at Berkeley about 1933 or '34. She had worked a great deal with Carl Sauer and was a close friend of the Sauers. She was living in Mexico at that time, mostly in and around Guadalajara. But I knew about her--she was in Mexico City

during part of that time--and we were good friends until she died about fifteen years ago. Everybody has died.

Riess: And Trotsky? Wasn't Mexico City a haven for him then too?

Foster: Yes. I don't remember whether he was assassinated while I was there, or whether it was just before or just after. It was about that time, though. I was so non-political, I wasn't paying any attention to it. I was intent on learning Spanish and learning culture and meeting anthropologists.

There was a--the Mexican Society for Anthropology, Sociedad Mexicana de Antropología, would have a meeting once a month in those days, in the evening. Afterward, everyone seriously anthropologically oriented would go to the Cafe de Tacuba, in the Calle Tacuba near the cathedral in Mexico City, or to the Cafe Bellinghausen in what is now called the Pink Zone, the Zona Rosa, for a late supper. There I became acquainted with a number of people, including Donald and Dorothy Cordry, Americans who had gone to Mexico about 1938 and lived there. A few years later they built a wonderful house in Cuernavaca, where they lived the rest of their lives. We were good friends, though we had our ups and downs. Donald Cordry was very artistic and a fabulous photographer. He had been with Tony Sarg's marionette troupe, making marionettes. One of the marionettes--who was it who had the dance of the seven veils?

Riess: Salome?

Foster: Salome. He had made a fabulous marionette. I think it had more joints than any other marionette to date. He was famous for it.

And we became acquainted with Miguel Covarrubias, who was a wonderful friend. We liked each other. Later on we made a wonderful trip in Mexico, to Tehuantepec, with him and his wife. I'll tell you about that at the right spot.

Riess: Not all of these people are anthropologists, not everyone you were meeting.

Foster: Miguel Covarrubias was an anthropologist, as well as an artist. He taught at the school of anthropology. The Cordrys could have been anthropologists, but they weren't. They were doing anthropological fieldwork. He published a book on Mexican masks, and one on costume in the papers of the Southwest Museum in Los Angeles. He was not formally trained in anthropology, but he was an anthropologist. Most of them were anthropologists. And Wigberto Jiménez Moreno, a Mexican historian and anthropologist.

Riess: Were they all somehow in love with Mexico and the Mexicans? I mean, what was the spirit of this group of people, would you say?

Foster: Well, the Americans were always delighted with Mexico. Frances (Paca) Toor was another, a Berkeley schoolteacher who had gone down to Mexico about 1925, at a time when the artists and the anthropologists were first aware of the uniqueness of Latin American culture. That must have been a wonderful period to be in Mexico. She started a little journal called *Mexican Folkways*--it came out irregularly. She would go to fiestas, and she'd write about them, and about food and customs. It was a wonderful little magazine. Ultimately she did a whole book based on these, called *A Treasury of Mexican Folkways*. She was there at the time. We became good friends.

Riess: This interest in the Mexican culture, how would it compare to the interest in the American Indians?

Foster: Well, most city Mexicans had nothing but scorn for the Indian population. They saw them as a drag on the development of the country.

Riess: The peasant population.

Foster: Well, those who were Indians, peasants in general.

But the anthropologists and the artists were aware of the richness of the various indigenous cultures, and they were interested in promoting recognition of Mexican arts and customs. There are a number of wonderful books from that period. There was an artist named Dr. Atl, whom I never met, who did a wonderful book on Mexican popular arts. I mention that as illustrative--he was not the only one, by any means.

So there was this very enthusiastic group of people who liked to get out in the country and see the people, and bring back museum specimens, mostly for their own homes. It was a wonderful way to get acquainted with the active people in the field that I loved so much. There were others, but I can't think of them at the moment.

Riess: I'm wondering if it was a romanticized relationship with the primitive people, whether that was part of it? Like what we might feel about the American Indian.

Foster: No, I wouldn't say so. It was more than that, because the Inter-American Indian Institute was established in 1940, and that is still a major factor in promoting the welfare of the Indians. In Mexico the anthropologists, unlike anthropologists in this

country, were from the word "Go" applied as well as theoretical anthropologists. Caso, who is the most eminent Mexican archaeologist of his time, was instrumental in starting the Indian Institute. So it's quite different from how Americans looked at the American Indians.

The difference is due to the fact that in 1940 in Mexico you had--even to this day there is a great deal of indigenous culture which you can see. In 1940 there were no communities in this country, other than the Southwest pueblos, where an anthropologist could go and really feel he was participating in the life, and even then there would be much more change in the Southwest pueblos than there would be in Mexico. In 1940, when I first went to the Popoluca, their customs were largely indigenous. The bridges they had--I remember a hammock bridge, a suspension bridge across a river. (I've got a picture, walking across that bridge.) The weaving of costumes, making raincoats the way they did in the pre-Conquest time, things like that, things you couldn't possibly see now, were still very much in evidence.

In any event, I was very lucky to be there as soon as I was able to go, in 1940. The golden age, as I've said before, is always the decade before ego arrives on the scene. I was sorry I hadn't gotten there a decade earlier.

Riess: "Before ego arrives on the scene?"

Foster: Yes.

Riess: What does that mean?

Foster: Ego is my generic term for I. I got here to Berkeley in 1935 to begin graduate work. The golden age was the decade before 1935, before I got here.

First Visit to the Popoluca, Spring 1940

Riess: How long were you in Mexico?

Foster: The spring of '40, from the 13th of January until the middle of May. Then I went down again about November of '40 and was there until the end of May of '41.

I'm getting to the point of telling you how I got to the Popoluca. It was at Easter of the spring of 1940 that Mr. Weitlaner, as I always called him, invited Enid Zacharias and me

to go with him and another student to an Aztec-speaking community called Acula, in Veracruz state, south of the city of Veracruz, where he was planning to spend about five days recording language.

Mickie had--Jeremy, our son, was a year old in March, March 18th. She had left him with her parents in Washington. Six weeks was about as long as she wanted to be away from him, so she went back before I went to Veracruz with Mr. Weitlaner.

It's amazing how long it took to get places in those days. I remember we left on the train about 8:30 in the evening on a sleeping car, a wonderful old--it was a British railway, the Mexicano. The sleeping cars were American, Pullman style, not the British compartment system. They had a potted palm, and a leaded glass skylight at one end of the car. They were so heavy that they were put right behind the engines, because if they were put at the tail of the train, in going around the sharp curves they would have been pulled off the track.

We arrived at Córdoba the next morning about 8:30, where we connected almost immediately with a local train that ran southward to Tierra Blanca, a junction with the main line of the Mexican National Railway going south and east, to Tehuantepec and the Guatemalan border. This was about a three hour run through the tropical morning. It was wonderful: mist hanging in the hills, and the lush foliage was just gorgeous. At Tierra Blanca we transferred to the southbound express train for a couple of hours, arriving at another junction called Tres Valles. Here we picked up yet another local train, and after three more hours reached the small city of Cosamaloapan, about 8:30, where we spent the night. We had been traveling twenty-four hours to do what today can be done in four or five hours. The second twelve hours, on three trains, took us only 110 miles!

The next day we took a car down to Acula, which was a wonderful town on the edge of a stream that went into the Bay of Alvarado, south of Veracruz. It was there that I learned to enjoy cigars. They made cigars in all the lowland communities. Mr. Weitlaner loved cigars. He was a wonderful person--just to watch his way of initiating contact with people he'd never met. I owe a great deal to him. He taught me how to do fieldwork, really, by example. He'd come upon a little store selling locally-made cigars and buy and smoke some. He had an easy, relaxed way of dealing with people. And he could get information--most of us would have to wait for weeks to get the rapport established he'd get.

Well, linguistic rapport is easier than others because language is not something that's hidden. There's very little

sensitive stuff in language. You can ask--they're not revealing confidence when they'd say, "I go, you go, we go," et cetera. We were there for about five days, and I became acquainted with a young man named Rafael Carrillo, who had worked with an American engineer, and he knew a few words of English. He couldn't really talk English, but he knew a few words of English.

While I was there, Mr. Weitlaner said, "George, why don't you go down and check on the Popoluca. We're close to where they live." He said, "That's the group of Mexican Indians we really know least about."

##

Riess: You were saying that the Popoluca had been visited in 1925.

Foster: For several weeks by two well-known researchers--Franz Blom was a Danish archaeologist, and Oliver La Farge was an American ethnologist. They were both very well known in anthropology. But they had very little stuff really.

Mr. Weitlaner said, "Why don't you go down and see if you would like to do your doctoral research there." And it turned out that Rafael Carrillo had been in that country for several weeks, on a trip with the American engineer, so he agreed to go with me.

We took the boat on down the river to Alvarado, the town, a fishing port, and we stayed there a day or two. Then one afternoon he and I took a boat across the bay to the south side, a trip of an hour or so--now there's a bridge that you go over--to a little town called Meson, where we spent the night.

The next morning we had to get to San Andrés Tuxtla. There was a dirt road for about ten miles, so we got on a truck and went ten miles. Then we came to a strip, about a dozen miles, where there was only a horseback trail. As luck would have it, two men came from San Andrés on horseback, and they wanted to send their horses back, so just at the opportune time God delivered these two horses. We rode until about three o'clock in the afternoon, I guess, and we got to Santiago Tuxtla, where there was an unpaved road going to San Andrés, about, oh, ten miles further along.

The only car in town was on blocks, so it looked as if it wouldn't do us any good. But they said they thought in an hour and a half they could get it rolling [laughter], and so they did, and they took us the last half hour into San Andrés, where we put up in a hotel that had a cigar factory on the first floor. The wonderful smell of tobacco was all through the bedrooms, which were rooms built of wooden planks over the tobacco storage rooms on the first floor, all very primitive.

The next day we went on down to Catemaco, Lake Catemaco, which was connected to San Andrés by a *brecha*, a word that means "breach" or opening (e.g., in a wall) but in Mexico is the word used for a single lane ungraded road, the first "breach" in the savanna or forest, over which trucks could make their way in the dry season. So we went down to Catemaco, where we had to get horses and a guide. As luck would have it, we found out the only muleteer who was willing to take anyone was named Enrique Hernández.

He, it turns out, had been the guide for La Farge and Blom fifteen years earlier! He was out of town, but they told us, "He's due back tomorrow." So we waited, and we talked with him, and he said, yes, he would take us. We hired him, and we spent ten days making a circular trip through this country on horseback. It was rough country, and I didn't even have a sleeping bag. All I had was a little thin cotton blanket.

Riess: This was what you were carrying with you, basically a blanket and a toothbrush?

Foster: Toothbrush and a shaving kit and a change of underwear, and that was about it.

Riess: Did you take a camera with you?

Foster: Yes, I had a camera. A movie camera, too. I can show you this trip on video if you'd like to see it.

Riess: And then notebooks and so on.

Foster: Yes.

Riess: Anything else?

Foster: That's all. Just what I carried on a pack on a horse, in addition to myself.

Riess: The trip sounds like a test of whether you really wanted to be doing this kind of work.

Foster: It was. I lost ten pounds in those ten days. We'd sleep in the attic of thatched-roof houses, going up a notched ladder to get there, and there was no mattress or anything, just the cane floor, bumps. I don't know how I did it, but I was raring to go back when the next year came.

Riess: You say "as luck would have it," like this trip was full of fortune, but it sounds very difficult.

Foster: Yes, it was hard work, but it convinced me that it was a wonderful place to study. So we came back. No anthropologist had been there since Blom and La Farge had been there. We were the first ones to go back.

I thought in 1940 I was able to identify all the villages where Popoluca was spoken. It turned out that I had missed a number of them. I did a little paper called--you have the list.

Riess: "Notes on the Popoluca of Veracruz" [Mexico, D.F.: Instituto Panamericano de Geografia e Historia, Publ. No. 51].

Foster: That was the account of this first exploratory ten days.

Return to the Popoluca, November 1940-Spring 1941

Foster: I took the train back, leaving San Andrés Tuxtla at 4:00 A.M. on April 12, arriving in Mexico City twenty-seven hours later. Several days later I left Mexico City in our Chevy sedan and driving fifteen hour days covered the two thousand miles plus to Ottumwa in only four days, fantastically rapid in those days of no superhighways. I then took the train to Washington where Mickie and Jeremy were staying with her parents, and after a few days there the three of us returned to Ottumwa, again by train, and a few days later left in the faithful Chevy for the five-day two thousand mile drive to Berkeley, arriving in mid-June.

I spent the summer and early fall of 1940 studying Spanish and revising my 1937 Yuki monograph for publication. In November, I drove back to Mexico City alone, a 2,800 mile drive that took a full week. At that time there was still only the road from Laredo to Mexico City, which meant that driving from California meant crossing Arizona, New Mexico, and half of Texas before entering Mexico.

Mickie came down shortly after the New Year began, in 1941. This time we were ready to do the bulk of the work, which turned out to be only three months--ridiculously little time by today's standards. We carried an umbrella tent. We carried some food, some dried food. We had blankets--not sleeping bags but blankets and big horse pins, so we made blanket rolls. Folding cots. In addition to what we could carry in our packs on our horses, we had a pack horse. I had been in touch with Enrique and told him I was coming back, and he was waiting for me when we got to Catemaco. This time we went on the train all the way to San Andrés Tuxtla and then just a half-hour ride by car to Catemaco.

We went via a different route than the preceding year. We planned to stay in a town called Ocozotepec, but we found the Indians even more distrustful than I had felt the year before. I remember we'd set up our camp and our fire and we'd bought a chicken from somebody, and some beans. Nobody would cook for us. There was one mother, a little more bold than some of the others, who came around with a small baby. We had--I think it was bean soup or something, or chicken broth, some simple thing, some kind of a dried soup. We asked her if she'd like to try it. She did, and she put it in her mouth and spat it out. And then she let her baby take some, and he just lapped it up. He just loved it. But it was something she wasn't accustomed to, and she didn't like it at all.

But after about three days, we realized that we weren't going to get anyplace there, so we decided to go to Soteapan, which is the *cabecera*, the head of the *municipio*, which is sort of like a township, a cross between a township and a county in Mexico. Mexican states are divided not into counties or townships but into *municipios*. They are larger than a township in most American states but a good deal smaller than a county, so there are more of them. Soteapan was the head town of the *municipio* of Soteapan, which was a *municipio* largely inhabited by Popoluca Indians.

I had a letter that I had gotten from the governor of Veracruz in Xalapa, the state capital. We had a letter from Caso also. I spoke to the municipal president, the mayor, and he said, "Well, we'll see what we can do for you." The first night we slept in the town hall, which was just a barn. We opened out our cots and spread out our blankets. Bats were flying back and forth. Mickie was kind of worried, she was afraid they were blood-sucking vampire bats. Later a fellow came in--there was a door lying on the ground, and he just slept on that, next to our cots, all night. He was from another town. So we thought this was really kind of primitive.

Finally the mayor said, "We can get you a house. It belongs to a man that's not here, but it's all right for you to move in." It was a house with stick walls and a thatched roof, and a bench along one side, built by sticks in the ground that were tied to the wall, and a raised *fogón*, a level cooking area about a yard square with about six inches of earth on a kind of platform. Rafael was also with us this time, and he slept in the house too.

We pitched our umbrella tent near the house where we could--there were very few bugs, really, mosquitoes, flying bugs. But we had a little more privacy that way because you could look right through the walls of the house. So that was our settlement, and

we found there was a woman who cooked meals for the occasional trader who came through. Her name was Juana, Doña Juana. We arranged to take the noon meal with her--no, I guess we had two meals, and we'd get our breakfast in the morning. In any event, we could get food and we could do a little cooking ourselves, though we didn't often do it.

Riess: Did these people remember being visited by Franz Blom?

Foster: I don't think they did. He didn't indicate any of his informants, as I recall. And the people I talked with had no recollection of him ever being there.

Riess: How did you present yourselves? You were their first anthropologists?

Foster: I suppose I said, "I'm an anthropologist. I'd like to learn how people live here."

We were lucky to be near a man named Leandro Perez, this wonderful informant that I mentioned. And Rafael Carrillo was very helpful, since he had been there before and he knew the lay of the land. He could make up for our shortcomings in language. The women spoke almost no Spanish at all. Most of the men were bilingual or fairly bilingual. I apologized for my poor Spanish. I remember one Indian apologized for his poor Spanish, too. So we met on kind of an equal playing ground. [laughing]

Working Methods, Informants

Riess: Did you establish patterns there for how you approached people, patterns for your way of working?

Foster: We just used the usual anthropological style of walking around, and stopping and talking with anybody who would talk with us. And if I thought the person would be at all receptive, I'd say, "I'd like someday to come by and talk with you about this, that or the other thing." Occasionally they'd say yes. Mostly they were very busy.

This was one of the hard things about doing fieldwork here. They were all involved in agriculture, at some distance from the village, which was on a narrow ridge. They went early in the morning, and when they'd come back in the late afternoon they were so tired, they weren't interested in intellectual activity.

Leandro Pérez, who became our good friend and best informant, agreed to come back early if we would pay him. I forget how much we paid him, but it was a pittance, although it was good money for him. I have never paid in Tzintzuntzan, but I paid in Round Valley--when I was doing the Yuki study. Everybody paid informants in California, so I wasn't shocked the way I would have been if I had been starting out from scratch. And it was fair to pay him. He was giving up his work in the field, and he enjoyed it, I know. We would laugh. He was very unusual among the Popoloca, who were a very solemn bunch.

We had other informants, from whom we got lesser amounts of information. I remember one night Mickie and I were in bed, about two o'clock in the morning, and I heard Rafael shouting, "George, George. Come, come. Help." I sprang out of my cot, in my pajamas, and I opened the flyleaf of the tent and went over, and it turned out he was having an epileptic fit. I didn't know what to do. I stood there. I knew I had to get something in his teeth.

I guess while I was doing that, our next-door neighbor, Fidel, appeared at the door. It turned out he was a shaman. He walked over and he looked at Rafael, and he began stroking his [Rafael's] arms, like this [demonstrating], and then he'd shake his own hands, as if trying to shake something off of them, some mysterious poison he'd picked up from contact with Rafael's bare arms. Rafael began quieting down, and presently he was in a deep sleep, and Fidel said, "Now, wait. I'm going to bring something to you." He went next door to his house and came back with a beer bottle full of a noxious brown liquid. He said, "Tell him to take a couple of tablespoons of this three times a day," or something like that "and he'll be all right." And he was. And he had no more trouble all the time we were there. I've always thought how relieved I was for Fidel appearing. Here's a professional. He knew what to do. I was happy to give him the responsibility.

Riess: Yes! Did he become one of your informants?

Foster: No. He was not a good informant. He had information that he didn't want to give. That's one of the differences between medical doctors in our society and other traditional medical systems in primitive societies. Medical knowledge is something people hang onto, they don't reveal it to others. It's a valuable resource to them. He wasn't going to tell us about his techniques.

Riess: But you come from another culture, so what difference does it make?

Foster: If I had been there long enough, if I had been there for six or eight months, nearly as long as I was in Tzintzuntzan, I'm sure he would have come around. I found in Tzintzuntzan, after they had gotten used to us, they realized that I was not competitive on any kind of knowledge, and they would be very forthcoming.

I think anthropologists always find this, that after you establish rapport, that people will tell you things they won't tell their best friends, because they like to talk about it, they like to get it off their chest, but they're afraid to. They recognize the anthropologist is from a completely different society, and is not competing and won't use the information against them.

Among the People, Mickie's Role

Riess: Did you and Mickie go out together?

Foster: She would come with me. Every day--we carried our own water for cooking and we had no one to help us. There were streams on each side of this ridge, about five hundred feet down, lower, and we'd scramble down, take a bath and wash our clothing, and bring water up to the house. I remember I had a folding canvas water bucket that they thought was the most marvelous thing in the world. The Indians would come around and look at it, touch it, and see that the water doesn't pass through. That was the thing that astonished them the most.

There was an airplane, a Pan Am flight that went over every morning about two o'clock in the morning, on its way from Mexico City to Mérida, I think. I remember asking an old Indian, "How does that plane stay up there?" He said, "Well, you know how it is in the ocean. The boats just float along, it's just like that. The airplane up there just floats like a boat." And that wasn't a miracle at all. But a canvas bucket that could hold water, cloth that would hold water, just blew their minds, and they'd come from miles around and ask to see that bucket.

Riess: And what about your baby Jeremy? What about life back home?

Foster: We left him with my parents that time. That's why we did such a short stint of fieldwork. Mickie just wouldn't be away for more than three months.

Riess: There was no question but that she would come?

Foster: Yes. She assumed she'd come.

Riess: Is this because she's an anthropologist or because she wanted to be where her husband was?

Foster: I think both. We always assumed that as anthropologists we'd work together. It was the way anthropologists worked. I think the fact that a lot of women aren't interested in this sort of thing is what accounts for the divorce rate among anthropologists, which is very high. It's not every woman that can take the field like that. I must say Mickie--she had gone camping with her folks from childhood, she's a great outdoor person. She's an ideal person for that kind of a life. Though she was very timid. Since then she has often commented about how she wishes she had been bolder and gone out and established rapport with some of the Indians, the women. But she was just kind of--well, it was a forbidding kind of a situation.

I could work on economics only so long. I'd go to the field and watch them cutting the brush to plant maize. But when we found out that Leandro was awfully good on stories, we got interested in the language and decided we'd do that, so every afternoon we'd work with Leandro from about three o'clock to five o'clock. I'd pay him every day. I'd look at my watch and I'd say, "Well, it has been two hours this time. It's five o'clock." And he'd go out and look at the sun. "No," he'd say, "It's 5:30." He thought he was accurate.

Mickie has an awfully good ear. She can distinguish the differences in sounds, and she can write very rapidly, so all the linguistic work we did there was recorded by her, and the stories that were the bulk of the--well, we analyzed that for the grammar we wrote on the Popoluca, the *Sierra Popoluca Speech*.²

Riess: When you were interviewing Leandro, would Mickie be in the same room?

Foster: Oh, yes.

Riess: Was that a problem for a male informant to have a woman listening in?

Foster: No, he didn't mind at all. We were in our little stick-walled house.

² *Sierra Popoluca Speech*, 1948, Smithsonian Institution, Institute of Social Anthropology, Publ. No. 8.

Riess: What other things did you learn there about how to do it? Did you have to relearn in every community? For instance, in that little stick-walled house, was it important that you be inside, or was it better to be sitting outside so everyone in the community can see?

Foster: Well, there were no walls around any of the houses. It just was the only place we could sit down.

Riess: You could have gone into the tent, for instance.

Foster: That was too small for a third person to come in, just an umbrella tent.

Riess: Well, I'm thinking about the envy issue.

Foster: That was not a factor at all. It was just a question of having a place where we could sit, and this bed that Rafael slept on was also the seat during the day, and we'd sit, and Leandro--the little benches are armadillos. They're a half log, hollowed, with the head and a tail of an armadillo carved at each end. There were handles so you could pick it up and carry it. They were only about that high [demonstrating].

I remember once we were asked, "How many chairs do you have in your house in the United States?" We thought, and we said, "About fifteen, I guess." "Pooh, nobody would ever have fifteen chairs!" [chuckling]

We weren't worried about envy then. There was nothing we had that the Indians wanted except that wonderful pail. I think we gave it to Leandro when we left.

Riess: I'm interested in the point in the relationship with the studied society when they turn and ask you about yourself, or about the number of chairs in your house. How long does that take?

Foster: I suppose after a month or two. I don't remember at all. Mostly they don't ask very much about your background. They assume it's so different, in the old days, at least. Today, of course, they've all been in California. Many of them from Tzintzuntzan have been in the house here. But with the Popoluca, they had no idea what it was like. They didn't even know what Mexico City was like, or Veracruz, the capital of the state. That was wild back country.

Riess: Would you carry pictures to show them? Or would there be a reason why you would not do that?

Foster: No, I just didn't think of carrying them.

Riess: To show pictures would be to unsettle the culture that you're looking at, wouldn't it?

Foster: No, I don't think so. First of all, people who have never seen pictures have a hard time interpreting them. It's not obvious. I remember I had a book once with a picture on it, and printing on the same page, and I was reading it, and somebody came and he said, "Let me look at that." So he looked at it and held it backward and upside down. I said, "Juan, can you read it?" He said, "Yes, I can read it, but I can't understand it." [laughter]

Writing the Dissertation, the Berkeley System

Riess: So you came back from that, and you wrote your dissertation.

Foster: I wrote it in jig time, in about three months in the summer of 1941. It's not a monumental job at all. I'm not at all proud of it. But it met the requirements.

Riess: And it was published.

Foster: Yes.

Riess: Was that requisite?

Foster: No, no.

Riess: How did you get it published?

Foster: I gave the American Ethnological Society a subsidy of four hundred dollars. Columbia University had the requirement that you had to publish your dissertation before you could get your degree. That was not changed until about 1950, as a consequence of which there were all kinds of anthropologists who had long since finished their doctorate, but they hadn't had the degree formally awarded because it had not been published.

Riess: But that was Columbia.

Foster: That was Columbia. Cal had no requirement like that, nor did we bother about a master's degree at Berkeley. When we entered the graduate school, we were going for the Ph.D. I don't have an M.A., for example, just a Ph.D. Now we routinely award it at the end of the first year, I think, so people have something.

Riess: What does that something net them?

Foster: Well, if they go through and finish the whole program it nets them nothing, except a couple of more initials after their names.

Riess: But if they don't, then they've got the master's.

Foster: Yes, they've got something to show for their graduate work.

Riess: I've read about the view of the dissertation as "an exercise in competence," in the Kroeber, Lowie and Boas tradition, or a magnum opus based on total immersion, in the Benedict tradition. Would you talk about that?

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Foster: So many graduate students feel their dissertation is their magnum opus, that they've got to have a monumental job. But Kroeber and Lowie didn't look at it at all that way. It was just the last in a series of exercises that were a part of our training. Bob Heizer, for example. I often think of Bob, who had done enough archaeology to have written three or four doctoral dissertations, but he didn't use archaeology at all. He did a library dissertation on aboriginal whaling in the north Pacific. I've had to apologize for my dissertation often, because it's based on such limited work and so modest in scope.

Riess: What does total immersion mean? Is it sheer number of years?

Foster: Today--and this really started with the British social anthropologists--it's assumed that an anthropologist will spend at least a year with the people he's studying, and it would be more like my first stint in Tzintzuntzan, where I had loads of material. Today students expect to get a book out of a dissertation, which is good because they'll have that for their appointment and maybe even their first promotion. So the dynamics have changed a good deal.

But even at that time--Carl Sauer, for example, his students had magnum opuses, some of them at least. Webster McBride, for example, who took his degree about the time I did, has a monumental magnum opus on markets in Guatemala. It's the last thing he ever did!

Riess: It sounds like one of the dangers is you would get too closely associated with that one work.

Foster: Yes. And there was a tendency for people, since they couldn't begin to include everything in their dissertation, rather than going someplace else or going back, they'd keep on drawing on their original body of data. And in a rapidly-changing world of

the last fifty years, by the time they would get it out, it would be out of date.

I like the Berkeley system, I think it was very good: viewing the dissertation simply as a progress report on the students' development, the last in a series of exercises. I have consciously tried to prevent my students from writing long, long dissertations.

Riess: So *A Primitive Mexican Economy* was published in 1942. And the writing? Has Mickie been helpful? Has she been your audience?

Foster: She has been very helpful. For the first twenty years of our marriage she would read everything I wrote in manuscript form. She's very good both on grammatical constructions and misspelled words. I wouldn't publish anything unless she had gone through it several times. But when she went back to graduate school she had her hands full with her own work, so I've been on my own the last thirty or forty years.

Riess: Were there other people to whom you would turn? I know there's the earlier story about Margaret Lantis.

Foster: Well, when we did the Popoluca grammar, it was my second year of teaching, 1942-1943, at UCLA. Harry Hoijer, a linguist and an anthropologist at UCLA, was there, a wonderful person. He taught us what we knew about linguistics. He told us about phonemes, we'd never heard about phonemes. So he was tremendously helpful in writing the grammar. He must have done a good job on us because that little volume was well received, and well reviewed. It was recognized for what it was, an attempt by young anthropologists to get data on record.

Riess: Your writing style is very clear and straightforward. Did that come naturally, or did you struggle?

Foster: Well, writing is never easy. My style is my own. I think it comes from having read a lot as a kid and having had this wonderful grammar course that I mentioned in high school. I knew not to split infinitives and a few other things. I'll read something I've written, and I'll ask myself, "Now, is somebody else going to understand that?" If I think they won't, I try to do it again.

Kroeber taught me to try to use as few words as possible to express an idea. He was a master at that. I remember Dale Stewart, a physical anthropologist at the Smithsonian, told me how apes brachiate, which means they swing through the forest on their arms. Dale Stewart said, "Kroeber in a few words describes brachiation better than anyone I have ever read." I was aware of

that. Kroeber's second *Anthropology* came out the same year that Herskovits's text on anthropology came out, in 1948. I compared the two. Herskovits, who was an excellent writer, as compared to Kroeber on the same topics, would use almost twice as many words.

I have tried to teach my own students that less is better, to write as tightly as you can, and organize. I think that's the thing that most of us have most trouble with: organizing. I've been lucky on that. I've somehow had the innate talent or the ability to see things that are similar, that go together. I hope I've been able to help students realize the importance of classification.

That was the beauty of Kroeber's first seminar, or pro-seminar, for new graduate students: Stewart Culin's monumental, 800-page *Games of the North American Indians* for the Bureau of American Ethnology Annual Reports (v. 24, 1907). Kroeber met with us on the first day of the seminar and told us, "Now, I want you to classify all of these games." On the last day of the semester he took our reports. We didn't see him in class in between. It taught us the importance of classifying. I think that was a marvelous exercise. I don't know if it's worth a whole semester, but it has stood me in good stead always.

I find most students have difficulty classifying and organizing their ideas, their thoughts, their data. So I think the fact there wasn't TV, or radio, even, in my childhood, that the only thing to do in the evening was read, and I read widely, served me well. I think I must have developed a sense of what sounds possible and what was impossible. I have always prided myself on my simple writing. I've never understood fancy words. I think that's one reason.

IV FIRST EMPLOYMENT, TEACHING

Teaching Sociology at Syracuse, Fall 1941

Riess: What about the war and the draft? What was your status, and what was your attitude about the war?

Foster: When the United States got into the war, I was married with one child and another on the way. I was teaching at Syracuse.

I guess I'd better get myself to Syracuse. I finished my dissertation early September of 1941, and I had no job prospects. One day Kroeber called me into his office and he said, "Would you like a job?" I said, "Yes." "Well," he said, "there's an anthropologist who teaches at an eastern university--not on the seaboard, not an Ivy League school--and he's going to be away for a year. It's in the department of sociology, a lectureship in sociology. You'll give sociology courses and one course in anthropology."

He asked, "Will you take the job?" I replied, "I'll take it. Where is it?" When he said, "Syracuse University," I said, "Oh, that's where the main line of the New York Central goes right down Main Street. You look out from the train in the middle of the night and you see you're going through Main Street."

Riess: How did you know that?

Foster: Well, I knew railroads, and I had been in New York a couple of times before, and that's the way you come to Chicago. And my father had been in upstate New York, in Schenectady, at General Electric, after his Pennsylvania work. I don't know, I just--I remember I thought it was most unusual to look out of the window and see yourselves going down the main street of a city on the Twentieth-Century Limited.

Riess: You were not going to turn down any job, but what about the fact that you were going to have to teach sociology?

Foster: I said, "Dr. Kroeber, I've never had a sociology course." His eyes twinkled. He said, "That's all right. Just teach anthropology and call it sociology." Well, it wasn't quite that simple.

I got to Syracuse. I flew in a DC-3, a sleeper plane, the first sleeper I had ever been on. I don't think most people recall that the DC-3, which carried twenty-one passengers normally, on United Airlines had a configuration like that of an old-fashioned Pullman car, with lower and upper berths. I got to Chicago the next morning and changed to an American Airlines DC-3. The plane made stops in Detroit, Buffalo, Rochester, and I got to Syracuse in the middle of the afternoon, after about twenty hours, having left Oakland the evening before.

At Syracuse I shared an office in the Maxwell School with a young sociologist named Charlie Bowerman, who was taking his degree at the University of Chicago, which had a very famous sociology department. Cal had no sociology at all at that time. Charlie and I became good friends. Douglas Haring was the man I was replacing. Doug, who started out as a missionary in Japan, had become interested in culture and personality. He was trying to be very scientific. He had written a sociology text called *Order and Possibility in Social Life* (1940), which we were required to use. But it was impossible.

Charlie Bowerman--he would clue me in on what I should talk about in class. He was very helpful. By the end of that first semester--the man in charge of the sociology department in Haring's absence was a sociologist named Tom Fisher. He recognized that the text was impossible, so for the spring term we used [William Fielding] Ogburn's text. He was a recognized sociologist at Chicago.

There were no big lecture courses. I taught for twelve hours a week, three sections of introductory sociology, on Monday, Wednesday and Friday at eight o'clock, at nine o'clock, and at eleven o'clock. The same course, exactly the same thing, same notes. The second time around was good because I could make up for the mistakes in the first lecture. But by the third lecture the words just cloyed in my mouth, I could hardly get them out.

I taught anthropology at eight o'clock Tuesdays, Thursdays and Saturdays. So in all I had six eight o'clocks. And the students were no more enthusiastic about Saturday eight o'clocks than I was. I had five students in that anthropology course.

Riess: Sociology was a new field, the same way that anthropology was?

Foster: I think it's about the same age.

Riess: Was it more popular?

Foster: Yes, it was more widely known. It was in more schools, except on the West Coast. Berkeley, as I say, had no sociology until after the war. That was an historical accident. But when anthropology was started at UCLA, it was anthropology and sociology, one department, which is what it was when I was teaching there my second year in 1942-1943.

Riess: What anthropology did you teach at Syracuse?

Foster: Sort of a standard introductory course. A little prehistory, a little archaeology, a little physical anthropology, and mostly world ethnography.

Riess: Did you like teaching?

Foster: I liked it from the word Go, yes, I did. I taught as a T.A. for one semester, and that gave me the confidence I needed, so I had no doubt that I could teach. It was like going to Mexico to do fieldwork. The fieldwork I had done among the Yuki gave me confidence. I knew I could do it by then, so I wasn't worried about that. I always enjoyed the teaching.

Riess: What were your family arrangements in Syracuse?

Foster: As I pointed out, I left Berkeley for Syracuse almost immediately. Mickie had to stay until--by then we had bought a house on LeRoy Avenue, which we owned until 1946. She had to make arrangements to rent the house. When Kroeber called me in, it was less than a week before classes began at Syracuse, so she came a couple of weeks later by rail all the way across.

We rented the upstairs of a duplex, the second floor of a house, the first floor of which was rented by a psychologist from Ohio State University named Ray Cullen, whose professor had been Mrs. Robert Lowie's first husband. Kind of amusing. It was a wonderful house for us because it was the only one in the city that had gas heat, so we didn't have to worry about having a furnace, and a furnace man. There were a couple of bedrooms, a kitchen, a dining room, a nice living room--it was an old house but well kept up.

The people who rented it normally probably paid fifty dollars. They took a place for thirty-five dollars, much simpler,

and charged us fifty-five dollars. For twenty dollars a month, they were willing to move out of that place, if you could imagine it. Money was--my salary was two hundred dollars a month, which was about what people were getting at other places at that time with a doctorate.

Riess: And at that time no other work was likely to materialize?

Foster: A year later, with the war on, there were all kinds of opportunities, but I was lucky to get that job.

Riess: What happened to the others who graduated with you?

Foster: Well, the only other person was Heizer, and Wally Goldschmidt came along the next spring, I think.

Riess: So Lowie's warning was entirely appropriate.

Foster: Yes, it was. There weren't jobs. The war made anthropology in the sense that it provided the jobs.

I remember Pearl Harbor Day. We used to listen to the New York Philharmonic Sunday afternoons. That was in the early days of radio, 1941, TV had not yet come at all. I was taking a nap, and listening to the music--I was in the bedroom, right off the living room--and it stopped, and there was this grave voice that came on and began telling about the bombing of Pearl Harbor.

Well, none of us thought--I mean, we knew we were in the war then. But as to what it meant for us individually, we didn't know. I registered for the draft. I must have done it in absentia because I was registered in Berkeley. My draft board office was down on Colusa Avenue, just off Solano Avenue. Before the end of the spring term, Ralph Beals had written me, asking if I would be willing to come to UCLA to replace him for a year. He was going to Washington to work with Julian Steward on the *Handbook of South American Indians*.

It turns out I could have stayed in Syracuse, which Charlie Bowerman did, for another year. But by the time they asked me to stay on, I had already told Beals I was coming, and I'd much prefer to be out there than Syracuse, which is pretty grizzly in the winter. Six feet of snow altogether I shoveled off the walk and the drive, and it was bitterly cold. Daylight savings time came on, and I remember I'd be halfway through my morning eight o'clock class and I'd have to ask a student to pull down the blind because the sun was coming up, getting in my eyes.

It was a good year at Syracuse. I had no regrets. I enjoyed it. I liked the people.

Riess: The East Coast. Did it feel like a different place to be?

Foster: Oh, yes. Having had a year at Harvard, I knew something about the East Coast, but Syracuse was quite different from the real East Coast. It's upstate New York, which is quite different. What struck me was all the homes and all the buildings were so old. A good many houses had had the front of the first floor extended to the edge of the sidewalk, and a small grocery or vegetable market would be installed in the new space. So much improvisation. In contrast, even in those days, California markets were purpose-built structures.

When I came to California I was dumbfounded to find that you could go out at three o'clock in the morning to a grocery store and buy bacon and eggs. I never dreamed of stores that stayed open all night. There weren't any in Syracuse, I assure you. So it seemed very provincial. But a beautiful countryside. I was sorry that we weren't able to get around--we had gas rationing almost as soon as the war started. And Mickie was pregnant, and the icy walks were not very safe for her. So we led a fairly sheltered life.

When we went to UCLA at the end of the summer of '42 we moved into the Beals' house. And we knew Harry Hoijer from--in those days anthropologists, by the time they got their degrees, knew most other anthropologists. I don't know where I met Harry.

Anthropology Meetings, Margaret Mead

Riess: You said that meetings for anthropologists meant quite a long trip, but you went to them?

Foster: Yes. In Berkeley I didn't get to meetings at all. It was during my year at Syracuse that I went to my first annual meeting of the American Anthropological Association, at Phillips Exeter Academy in Andover, New Hampshire, between Christmas and New Years. I met Ruth Benedict and Father [John] Cooper and probably most of the anthropologists that I didn't know previous to that time.
Margaret Mead.

Riess: I'd be very interested in your meetings with Ruth Benedict and Margaret Mead and the kinds of things you would talk about.

Foster: Ruth Benedict was very deaf by the time I knew her. She wore her hair short, brushed back to a point, and she'd look people very closely in the eyes, looking at their lips, I think. She looked like an eagle, or a bird with a cockade, just peering intensely. I can't remember talking with her about anything.

Father Cooper, I remember, was very gentlemanly. I came to know him better in Washington when I was there at the Smithsonian. He was the head of the Catholic University anthropology department.

Riess: And Margaret Mead?

Foster: I'm not sure she was there at that time, on that occasion. She probably was. I don't remember her at that time. She sat in that [gesturing across the table] chair once, when she came through Berkeley and I was chairman of the department, about 1958. I remember I invited David and Hilda Krech to have dinner with Margaret Mead.

David Krech was an original, also, he had the funniest mannerisms. He'd look at her and say, "Now, Dr. Mead, what do you think about this?" She'd come back with some reply--she gave as good as he gave. Very amusing, though. And she was a very strong character.

Riess: Krech was a psychologist, wasn't he?

Foster: Yes.

I, of course, interacted a good deal with Margaret Mead over the years. I knew her very well.

Riess: Where would you interact?

Foster: Oh, committees, for example. My last interaction was when I was chairing a committee of the American Anthropological Association, about two years before she died. It must have been about--well, I guess in the mid-1970s. I can't remember what the committee was about, but I remember she gave me hell because she said, "Now, you should have the draft of the manuscript prepared with numbered lines." She told me where I could get paper with number lines.

I said, "Margaret, I've got to do it my way. I'll give you a clean draft, and you can do anything you want with it." She just gave me hell. I laughed. If she thought I was going to give her numbered lines after that she was badly mistaken. [laughs] We got along fine.

Riess: She was the consummate woman anthropologist?

Foster: Well, she took time to build her strength. I don't know whether she would have built it in a regular department or not. When she wrote *Coming of Age in Samoa*, Kroeber reviewed it in the *Anthropologist*. I remember it. He said, "This is a promising young woman who has interesting ideas. It would be nice if she gave us more data."

For a long time, Margaret Mead was not—she was well known, but she was not famous, nor super-respected. She was respected like anyone else, but she was not a particular leader, I would say. I think because of her personality she never really had a regular ladder job in anthropology. She preferred to lecture. She had her office in the American Museum of Natural History, where she was a curator, I suppose. She could have had any number of jobs if she'd wanted them, but she preferred to lecture and write on popular topics.

Riess: She entered the public imagination.

Foster: Yes. There's been no other anthropologist remotely approaching her. She made anthropology for Americans what it is. They know anthropology because of Margaret Mead. She achieved recognition when she was elected to the presidency of the American Association for the Advancement of Science. She was a remarkable person.

The War, Draft Board

Riess: You said you went to your draft board. And you were ineligible because of your family status?

Foster: I didn't know that, though, until the middle of spring of 1944. I expected I'd be called up at any time.

At one point, when we were spending the summer in Washington, I went to the navy and asked about what I might do. I thought--what do they call it, breaking of codes? Cryptography. They gave me some material on which I tested myself, and I found out that that was not my *métier*, I had no business in that field. So I didn't do anything else. I just decided I would wait, and when they called me, I'd go and hope that I'd get in something where I could do some good.

Riess: Were your brothers in the war?

Foster: My second brother, Bob, was 4-F from the beginning. He was very deaf from double mastoid twice as a kid. My third brother, Gene, was at Stanford, in the class of '43. He was hurried up a bit, like all the engineering students, and he went into the navy and was the chief engineering officer on a destroyer escort in the Pacific. After that--well, he was a year in the Pacific, on a destroyer escort. He's the only one of the three of us who was in the service.

Riess: Next we have you at UCLA teaching anthropology.

Foster: I enjoyed that because I had only, I think, two courses. Maybe I had three--I can't remember any seminars that they gave there. But I just gave standard courses. Kroeber's famous course on the history of culture I cut down to one semester, I think. And I gave world ethnology and general anthropology, and probably a course on social organization.

Riess: Would your exams have looked like the Kroeber or the Lowie exams?

Foster: I don't have any copies of my exams from that period, I don't know. Later, I shifted to essay-type questions. I never liked the kind of exams that Kroeber and Lowie gave us.

Riess: And yet you valued what they were trying to do.

Foster: Yes, I did, but not enough to want to do it myself. Also, it's a lot of work, making up an exam like that. You can spend the time making the exam and have a quick grading system, or you could have a lot of time reading an essay.

UCLA's Department of Anthropology

[Interview 4: December 10, 1998] ##

Riess: What was the status of the anthropology department at UCLA when you arrived there?

Foster: It was a part of the sociology department. They had been established together. Ralph Beals established them, and as long as he lived and was active, he refused to let them separate the two departments. Psychology was a separate department, but it was almost the same because the three departments were in the same building. I can't remember the name of the building. It wasn't Royce Hall. Altogether, there weren't over about eight people in the three different fields. Very small. Delightful.

Riess: What was delightful?

Foster: The small size, the feeling of comradeship. The hilltop was almost bare except for Royce Hall and a few other halls. It had occupied the site only since 1929, so by 1942 it had grown some but not much.

Riess: What was that year for you? What did you particularly accomplish? Maybe this was your first real teaching in anthropology, so did you develop what became a signature course?

Foster: I largely regurgitated the courses I had taken at Berkeley. I had three courses each semester, and when you're starting out to teach, that's nearly a full-time job.

Harry Hoiyer was particularly helpful. Mickie and I--I think I mentioned that when we were with the Popoluca, we got a lot of linguistic data that we didn't really know how to handle. Harry, who was the chairman of the department at the time, in Ralph's absence, was a linguistic anthropologist, and we learned a great deal from him. He was most encouraging, very helpful. I've always been grateful to Harry. The result was our little grammar of the Popoluca Indian language, which was published by the Institute of Social Anthropology in 1948, and which had a remarkably good reception. That's what gave Mickie the confidence and the interest to go on and be a linguist when she took her doctoral degree here at Cal.

Harry was a wonderful fellow. His father was a streetcar conductor in Chicago, a motorman. He [Harry] told me how when he was in high school he went to a friend's house and was astonished to find books. He grew up with no books in his house. I think that's the great thing about this country, the way people who have the basic ability have an opportunity they have no place else in the world.

Riess: The public schools.

Foster: Public schools and public libraries, and the fact that we don't hold people back because of social background or economic background.

Socializing at Berkeley, Little Thinkers, Et al

Foster: On this point, I was much interested on one occasion at the Little Thinkers, the Friday lunch group I've eaten with for many years.

This particular Friday David Krech brought C.P. Snow to lunch, and we got on the subject of social classes. I said that in this country, the U.S.A., social classes are much less marked and much less important than in Britain. In this country, I continued, we don't really know the social class from which our colleagues come; there are no barriers, serious barriers, at least for WASPs.

C.P. Snow replied by saying there are very few barriers in Britain, too. People come from all kinds of backgrounds. He mentioned someone--I believe it was the physicist Faraday, whose father, Snow said, was a coal miner, or something like that, as an example. I thought to myself, that's interesting; he says it's not important but he knows the exception to the rule, which indicates that it *is* important, if you follow me.

Riess: Yes.

Foster: The fact that he knew that.

Riess: We assume their system stops people at all kinds of points.

Foster: Well, it does in Europe in a way it doesn't in this country. You don't get a second chance in Europe. We do. Our education is much less thorough than the British and the Continental education systems, but in the long run I think it's more flexible, and a person can come back and find himself much later than in Europe.

Riess: Who are the Little Thinkers?

Foster: That's a group that was founded about 1948 by my colleague, David Mandelbaum, and Van Dusen Kennedy, and a couple of other people who were interested in India. They got together to talk about India and working with students, and they met every Friday. Little by little, they brought in a few other people until it became a regular group. David brought me into it in 1955. Van Dusen Kennedy is the only original one still in it. I think I'm number two in my seniority.

Hilda Krech named the Little Thinkers. She said, "My father always said he belonged to a little group of distinguished thinkers. My husband belongs to a distinguished group of Little Thinkers." [chuckling] The name just kind of stuck. It has been a wonderful group. Every Friday we still meet. We had--Catherine Bauer [Wurster] is the only woman who was ever a member. And let's see, Stephen Pepper was a member, Herbert Blumer, Henry Nash Smith, Al [Albert C.] Pickerel, and Alex Meiklejohn was a member when I joined. Joe Tussman is still one of the regulars, and John Reynolds is a regular.

Riess: What would you say is its political composition?

Foster: Initially it was very liberal. I was the token conservative. Now I'm the radical left! It has changed so much. Krech and Henry Nash Smith were always very liberal in their viewpoints. Over the years, the people have become more and more conservative. I think they still all vote Democratic. Or most of them do. Joe Hodges is the enigma. Al Bowker is now a member. He comes regularly.

Riess: Is there any kind of record of their meetings, of the discussion?

Foster: No, no. The only thing we're not supposed to discuss is something we know something about. [laughs] But that doesn't always work out. It's a very interesting group. And all kinds of interesting topics.

Riess: Are the topics designated ahead of time?

Foster: No, no. Everything is spontaneous. Nobody gives a talk at all.

Riess: How does the conversation begin?

Foster: Whatever anyone brings up. Joe Tussman made some interesting comments about two months ago. Talking about the Clinton case, he said, "In this country we spent the first two centuries separating the religion of a politician from his performance in office." He said, "Now it's agreed that a man or woman's religion is of no bearing if he or she is competent in office." He said, "It looks like the next step is going to be his personal life." I thought that was a very interesting observation. I think that will probably prove true, too.

Riess: Is it an argumentative group?

Foster: All opinions are by no means the same. It's always stimulating. Everybody has something to say.

It's a good window on academic minds. Over the years we've represented a number of fields. Mark Rosenzweig and Sheldon Korchin and Krech in psychology; Burton Benedict and Sherry Washburn--but he's out of everything now--and me in anthropology; Joe Tussman and Pepper from philosophy; John Reynolds from physics, Marvin Cohen from physics; Don Foley from city planning, Van Dusen Kennedy from the business school. These are pretty much the regulars, and collectively the knowledge that group has just blows my mind.

Riess: Does the university claim it, or is it totally independent?

Foster: Its only formal recognition is on the Faculty Club bulletin board. On Friday it says the Little Thinkers are meeting in this or that room. Usually it's the Hart Room, which has about a dozen seats, fourteen at most. And members show up, or not.

It's not like the Kosmos Club, which has gone on for almost a century. That has a secretary, and people are lined up as speakers in advance, and the minutes are read. That's a different kettle of fish. This is totally informal and spontaneous. There's no election or anything. Anyone can bring anyone, and if the person likes it, they sense it and they stay and become members.

Riess: You can elect to make yourself a member?

Foster: Well, no, not exactly. But if you're taken by a friend and you enjoy it and decide you'd like to be a member, chances are very good that next time you are there nobody will--there's no formal approval of anyone.

Riess: I've never heard of the group before. And meeting once a week. That's a very vigorous group.

Foster: Just for the pleasure of getting together on Fridays. For the same reason that Elizabeth Colson and Desmond Clark and Burton Benedict and Bill Shack and Sherry Washburn and I meet regularly on Wednesdays, informally totally, in the bar.

Riess: In the bar?

Foster: Yes, in the Faculty Club bar, that's where we lunch. We like Paul Parish, who's in charge of the bar. I used to like a martini or a glass of wine with my lunch. Bill Simmons introduced us to the bar as a place to eat. I didn't realize the bar served lunch, but it's a nice place to sit and have a glass a wine and talk with a few friends. Paul Parish is a very interesting person. He was a Rhodes scholar and a friend of Clinton's. He knew Clinton in those days, in Britain. He joins in the conversation. He's extremely knowledgeable.

Riess: He's the host of the Faculty Club?

Foster: He's the bartender.

He loves ballet. He's a ballet dancer, and he writes articles on ballet. He occasionally goes to Europe on ballet trips. He's invited, and his expenses are paid. I think he enjoys the company at the bar. He's got a fine mind. Knows a

great deal of literature and poetry, so it's interesting to have Paul join in.

Riess: Has the Little Thinkers ever taken a position on anything as a group?

Foster: No, never. I don't think they ever will. We've reached a point now where I don't know if the group will last. We're all retired. When you get a group like that, you suddenly realize you're no longer of interest to the young people. I remember there was a club called the Berkeley Club that met on the campus. Jim LeCron, my father-in-law, was an outside member, and Dan Dewey, who was the mayor of Berkeley, was the last person that joined. And Jane Richardson's father, who was a professor--I can't remember what his field was--was a member.

They faced the same problem. They hadn't taken in enough members when they were a vital group. They invited me, thinking I'd be just enchanted to be invited. I went once and thought, This is for the birds. I'm afraid the Little Thinkers is the same way. The Kosmos Club itself is right at the point of no return, I'm afraid.

Riess: The Kosmos is a Berkeley group?

Foster: Yes. At one time it was a very prestigious group. It formally meets once a month, the first Monday of the month, about seven times a year, I think. The recording secretary does the humorous recording of what the speaker says. Jock Anderson for years was the recording secretary. He did a superb job.

Steve Diliberto is the long-time secretary who sends out the announcements, lines up the speakers. But we haven't been bringing in young members. I brought in a number, but they just don't seem to return. Lincoln Constance and Murray Emeneau were regulars.

Riess: On this subject, are you a member of the Bohemian Club?

Foster: No, never have been and never wanted to be. I'm a member of the Cosmos Club in Washington. I joined when I was in the Smithsonian Institution. Alexander Wetmore, the secretary of the Smithsonian when I was there, was one of the stalwarts. He said, "I want my boys to be members of the Cosmos Club." All the young anthropologists were members. Gordon Willey and Phil Drucker and Matthew Sterling, Matt Sterling, who was head of the Bureau of American Ethnology.

The Cosmos Club was a wonderful place, still is. Until about 1950 or '49 its clubhouse was the Dolley Madison home on Lafayette Square, almost opposite the White House. Then, when the club began booming after the war, they bought Sumner Welles' old mansion on Massachusetts Avenue, just to the west of Connecticut Avenue at DuPont Circle. I used to stay there often when I'd go to Washington on trips as an anthropologist.

When I was at the Smithsonian, we'd often go there for lunch, and I kept up my membership because it's--well, there's a certain degree of prestige, and it's fun to go there. I remember my mother-in-law, Mickie's mother, said, "That's the kind of club you put in your *Who's Who* when you're a member there." I didn't realize it was anything special at all. I've learned since that I was very lucky to get in. I've put up several people for membership. They've all been approved, happily.

Riess: Did you get anything out of the Little Thinkers group that perhaps contributed to your ideas in anthropology?

Foster: Well, I think one idea is the fact that a group has to have a formal mechanism for renewing itself. I tied that in with my Berkeley Club experience, and the Kosmos Club, the fact that a vigorous club life doesn't mean it's automatically going to go on forever.

I think the variety of opinions I got, political opinions that were quite different. I was quite conservative in my youth--I've told about coming from a staunch Republican background in small-town Iowa. And here were Krech, and Henry Nash Smith, and others, Stephen Pepper, Herb Blumer, all very liberal thinkers. I can't really point my finger at any one thing, but I think the discussion contributed to my breadth of views.

Riess: Wasn't that a frustrating rule, that you couldn't talk about your own work?

Foster: That was just sort of a joke. We don't formally talk about our own work, but if it's relevant to what's being said, we certainly don't hesitate.

Riess: When you were developing your paper on envy, for instance, might you have brought that up for general consideration?

Foster: Nobody is allowed to speak that long. I shouldn't say "allowed," but somebody else always cuts in. A couple of minutes is the most anyone speaks uninterrupted.

There is a social science group, club, which I've never belonged to, where people read formal papers, I believe. But I was never asked to join, and I never tried to join. I know very little about it.

Riess: I wasn't thinking so much that you might read the paper but that you might bring it up as a topic, throw it out, as it were, to see what common or uncommon sense there might be in that group of how envy functions in anyone's culture.

Foster: Nobody throws out a topic like that, it never happens. We're there to enjoy our lunch and the company, and whatever anyone brings up that's interesting to the group, we all join in. Otherwise, not.

Riess: Maybe you wouldn't throw it out because it's a precious new idea and you wouldn't want to discuss it prematurely?

Foster: No, that's not it at all. I think anyone is grateful for any ideas that he gets from the group.

For instance, I was interested generally in the religion of the group, so one day I asked people to put up their hands, and I said, "How many of you consider yourselves to be out-and-out atheists?" Well, there was a kind of a gasp, and Burton Benedict finally put up his hand. He was the only one. I was astonished.

I asked, "How many of you consider yourselves to be agnostics?" All the rest of the hands went up. I said, "I'm sure some of you are out-and-out atheists, but you just hate to admit it." For a long time I hated to admit that I was an atheist. I said I was agnostic for years. I think that's kind of interesting. That's the only time I've ever seen a show of hands on anything.

Riess: Makes me wonder, if you had asked it the other way around, what the results would have been.

Foster: Everyone would have been an agnostic probably, including Burton. When I pushed him, he said, "I really don't believe in a hereafter, a personal hereafter. It's just hard to say, 'My religion is atheism.'"

Riess: Were there any parallel social structures at UCLA, so that you got to know an interesting group of people in that year?

Foster: Yes, it was a very good year because I had more interdepartmental contact there than I've had anyplace. Leonard Broom in sociology was a very good friend. And his wife, Gretchen. A few years

later they moved to Australia and have lived there ever since. There were a couple of psychologists. There was a sociologist called Constantine Panunzio, who was quite a radical person. He caused a lot of trouble for the department because--I can't recall the story. I remember, though, he was a very interesting person, I liked him. But I could see how if I had been chair of the department, I wouldn't have been happy with him.

At that time there was no faculty club at UCLA. Several tables on the second floor of the students' dining room were reserved for lunch for faculty, so I'd sit down at a table there and meet people from various departments. I don't remember very well who they were, but I always thought it was very good to be able to sit down and have lunch like that with people from various departments. Basically, my social environment was the sociology, psychology and anthropology group.

Riess: What about the fact that there was a war going on? Was that very present in everyone's thinking?

Foster: It was, because the Douglas Aircraft factory was almost adjacent to the university, and sometimes I'd have to stop lecturing when planes would take off or land. Gasoline rationing was in full force--I forget how often we got four gallons. Around Los Angeles, you didn't take many pleasure trips. I rode the bus in from Beals' house in Santa Monica every day and back.

Yes, we were quite aware of the war. The fact nobody knew how long they'd be anywhere was unsettling. For me maybe less than others because I knew I had come there for only a year, but no one knew, the younger people, when they might be called up, when something would happen.

V WASHINGTON (AND MEXICO)

Analyst for the Institute of Inter-American Affairs

Riess: When did Washington come up? You knew you had one year at UCLA, but did you know what next?

Foster: No, not until about the first of April, when I got a call from an anthropologist named Willard Park. He had taken his degree at Yale--he had come to Northwestern University the last year Mickie was there, so she knew him. And his wife, Susan, was from San Francisco. Somewhere along the line, I had become acquainted with Willard and Susan.

Willard had a job in the office of the Coordinator of Inter-American Affairs, Nelson Rockefeller, in Washington. The organization was called the Institute of Inter-American Affairs [IIAA]. It was the first technical aid foreign program that our government ever had, as far as I know, in the modern period, at least, if you consider the war as modern. Willard had this job, and he was looking for staff members, so he hired me. He said, "Would you like to come to Washington?" I said, "Yes, I would." Because Mickie's folks were there, and we didn't have to worry about finding a place to stay.

As soon as school was out that spring of '43 we went back to Washington. I shared an office with Al Lesser, Alexander Lesser, who was a Boasian student who'd finished his course work at Columbia about 1935, I think. He didn't yet have a formal degree; he didn't receive his degree for ten years or more because at that point Columbia insisted that everyone have his dissertation published before the degree would be formally awarded. When they changed that rule there was a great flood of Columbia Ph.D.s--one year, thirty or forty.

He was a very stimulating, very interesting fellow, by my lights very radical in his thinking. But I had a great deal of

respect for him. He had a very keen analytical mind. I remember particularly arguing the class system with him. He insisted there were three classes: the upper class, the middle class and the lower class. And I insisted that it was just a continuum, that you could divide it four or five or two ways. We never resolved it, but we got along famously and enjoyed each other. His wife, Virginia, was a very fine woman. We often went out to have dinner together, or went to each other's homes.

Riess: When you started, what were you doing at the Institute?

Foster: Well, Al and I were put on a job to do a report on Haiti, or the Dominican Republic, I can't remember which, for background, for American government personnel going there. We consulted all the standard sources and put together kind of a synthetic statement, from public sources, and it came out stamped "Classified. Not for General Distribution"--it made me wonder about all sorts of classification. That's the only thing I remember we did--we worked the whole summer on it.

Riess: What do you mean it made you wonder about classification?

Foster: Well, the government falls over backward trying to keep documents out of the public eye.

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Foster: This is a good example of the way the system works. These articles were in journals like the *American Anthropologist*, American economics journals, and there was nothing in the least sensitive about it, and yet, to be on the safe side, somebody stamped it "Classified." I think that's the way bureaucrats operate. If there's any doubt, they take the very conservative course, and they do great damage by so doing. I began to learn a lot about bureaucracy, which has been an interest of mine ever since.

Riess: What do you mean by damage?

Foster: I was thinking of when I was running the Institute of Social Anthropology and we had to get clearance for one of the men we wanted to send to South America. That was the first time I had ever seen any of the government's personal security documents. For a long time I was afraid I wasn't going to be able to get this man cleared. He was of Finnish extraction and was working in the Southwest, and in his dossier were statements by the people who lived next door saying, "Dark-skinned men came to the house and spoke in a foreign language." That was in his file, and it was something the security agents pointed to, indicating their doubts.

I said, "Look, this guy is just as dangerous as I am, which is to say not dangerous at all."

Riess: Tell me more about your work at the Institute.

Foster: My job was called "social science analyst." I think my salary was about thirty-five hundred dollars a year. I can't remember. Quite an increase from the twenty-seven hundred I was getting at UCLA, which in turn was a vast jump from the eighteen hundred at Syracuse. I doubled my salary in two years.

Anthropologists had worked in government relatively little. John Collier made major use of them in the Indian Bureau. A psychiatrist named Alexander Leighton, who was an anthropologist also, was in charge of some or maybe all of the cultural aspects of the Japanese relocation centers. And Elizabeth Colson on our Berkeley faculty, and a number of other anthropologists, worked in these centers studying the reaction of the people.

And anthropologists have been used in the American Southwest by the Department of Agriculture, beginning about 1934 or '5. But this was classified as applied anthropology, which is not a field that anthropologists generally followed. In fact, we were trained to despise applied anthropology. The war had the positive effect of making American anthropologists aware of the possibilities. The Society for Applied Anthropology, for example, was established in, I believe, 1942, and it has been a vigorous organization ever since. But I didn't join until about 1950. I would have nothing of it.

Riess: In terms of your career, I'm curious about whether you had a grand plan--and how the time in Washington fit with the plan--a plan to get back into academia, and where field research was going to fit.

Foster: I had no grand plan at all. I was just like a leaf on a wave in a storm. I wasn't trying to get anywhere. I just wanted to hold my family together as long as possible. I don't think many of us were making plans at that time.

Riess: Because of the war.

Foster: Yes, it was not a time when--we weren't worried about not having a job, because there was a shortage of people everywhere. But very few people--I don't know anyone who was making definite plans for when the war was over, other than to try to pick up and go on.

Background of the Institute

Riess: Why did Nelson Rockefeller put the Institute together?

Foster: Well, he was aware of the fact that Latin America was very important to us. The IIAA was a war-born measure, of course. It brought to our consciousness the need for better relations with Latin American countries. It was part of "the Good Neighbor policy," which is a term that nobody uses anymore, but it described our attitude toward Latin America. We wanted to be good neighbors. We wanted to help our neighbors in Latin America.

There were three different branches of the Institute. One was agriculture, where it was argued that food supplies would be disrupted and the supply of rubber would also be disrupted, so it was essential to maximize agricultural knowledge and get rubber growing back in the Amazon--tapping the rubber trees in the Amazon had been largely abandoned after the success of the first rubber trees in Malaysia. Synthetic rubber was not yet very far along. So that was one branch of the IIAA.

The second branch was education, and the third branch was public health, which is the branch I got into, which we'll talk about later. But at this time, the Institute had been going only about a year. It was feeling its way. I think in all technical aid there has been a tremendous waste of money. I sometimes wondered if we had had no technical aid at all how the world would be different. I'm not sure it would be very different.

Riess: In general, or that particular administration.

Foster: In general. That's when I'm in a pessimistic mood.

So many people were in it for what they could get out of it. Washington is a wonderful place to see how people operate. They get an idea, and they see that it will boost them along, and they sell ideas, and an awful lot of ideas are of no use. Well, that's enough said now.

Riess: In *Traditional Cultures and Technological Change* you write about the anthropologist studying the studiers, or studying the bureaucrats, observing both the subject and the interaction. Was that new? Did that come out of your suspicion that there was an issue here?

Foster: My views about the nature of government bureaucracies began, I suppose, at this time, and developed over many years. I think they've culminated in a paper I wrote in 1987 in which I suggest

that there are three stages of awareness of the problems of passing our knowledge to people in other societies.¹ The first I call the "silver platter stage," which is what we were using then. This is the stage when you assume that if you have something that works in your country, you can give it to anyone else, regardless of cultural factors, and they'll see the advantage immediately and be delighted to take it up and adopt it.

The second stage we really ought to discuss when we get to the Institute of Social Anthropology.

Riess: Did you ever meet Nelson Rockefeller, talk with him?

Foster: I never did. My father-in-law worked for him and was a good friend of his. Mickie knew him quite well, but I never met him.

Julian Steward and the Institute of Social Anthropology

Riess: How important were your connections in Washington with Julian Steward? You had already known him, hadn't you?

Foster: My connections with Julian Steward were basic to the whole ISA experience. I knew him because he had been a Berkeley anthropology Ph.D. He had finished by the time I came to Berkeley.

He was an operator, in the good sense of the word, in the Bureau of American Ethnology, which was an old-fashioned government bureau, doing research that had to do with traditional theoretical problems (archaeology and ethnology and linguistics) having to do with the origin and spread over North and South America of the Native American people. He recognized the need for a general book, *The Handbook of South American Indians*, which would summarize our knowledge up to the early 1940s about all the areas and all the indigenous peoples in South America. That was one of his projects, and he brought Ralph Beals to Washington to work on it with him. It was a very successful undertaking, resulting in a marvelous six volume set (1946-1950).

The Institute of Social Anthropology was his second major idea, and his thinking there was also excellent. He recognized that after the war there was going to be a great change in all

¹"Bureaucratic Aspects of International Health Organizations," *Social Science and Medicine* 25, 1987.

parts of the world, that modernization had barely begun. He thought it would be highly desirable to have social scientists, especially anthropologists and cultural geographers, trained to do research on a whole series of developmental problems.

His thinking was stimulated by the experience of Donald Pierson, a University of Chicago Ph.D. in sociology, whose dissertation was on the port city of Bahia, in Brazil. It was published by the University of Chicago Press as *Negroes in Brazil*, a title that always embarrassed him, because the title implied that the book dealt with the topic nationwide whereas it was limited to the city itself.

In any event, Pierson had been hired by the Escola Livre de Sociologia e Política in São Paulo. This was a very interesting university-level school, established by a successful Brazilian businessman named Cyro Berlinck, who once told me the story when he came to Washington. About 1935 or '36 São Paulo, which has always been more progressive than the rest of the country, demanded a degree of autonomy from government regulations that almost led to a revolution. "Fortunately," he said, "we lost; we realized we were ill-equipped to bring about the changes we had in mind."

So the Escola was established to train people in modern techniques of management, and in the understanding of human behavior. It was a very dynamic institution, with sociologists, psychologists, economists, and business people on the faculty. I don't recall the circumstances that led to Pierson joining the faculty, but when in 1940 Steward first visited São Paulo he was there teaching sociology. That's what, he said, gave him the idea of a series of clones of Pierson who would teach modern behavioral science in universities and other schools of higher education in various Latin American countries.

So he set up units in several countries. The first was Mexico. These were organized by means of formal convenios, or small treaties, between the Smithsonian Institution and the ministry of education of the countries concerned, so that when we went, we were not just going as regular exchange professors, we were going as representatives of the U.S. government, and we were dealing with their governments.

Units were set up first in Mexico, second in Peru. I think Brazil was third--well, the order doesn't make any difference--and the fourth was Colombia. They were the four countries which over the years had exchange professors from the Smithsonian. I was the first person hired to go to Mexico. I was the first employee of the Institute other than Julian, and the last.

A Waiting Period

Foster: Julian asked me if I would like to go to Mexico. He told me if I did, I'd have to work among the Tarascans, which delighted me because I had already been in the area as a tourist in 1940, so I accepted. But I said, "I can't promise you because I'm subject to the draft." I was with the Institute of Inter-American Affairs for only about four months. In September I went to the Smithsonian, expecting to go very quickly to Mexico. But it took months and months and months for the convenio to be signed by our government and the government of Mexico.

Mickie and the two kids had gone out to Berkeley, where her parents had retired, thinking that I'd be out there, and that if we were going to go to Mexico I'd come there first. And if I were going to be drafted, that's where they'd stay. We had a house on LeRoy Avenue at that time, so she stayed there with her parents, I think. In any event, the whole winter dragged on.

Riess: This is the winter of 1944?

Foster: Forty-three-'44. About March or so, she came back to Washington, and I got my draft call. I figured I'd be drafted, I was in good health, so she and I drove back across the country to Berkeley. I reported to my draft board and was sent to the city for my physical. It was in the spring, when pollen was in the air, and I was wheezing when they tested my lungs, and they asked me about allergies. I said, "Yes, I've had bad allergies as a child: ragweed." After the test they brought us all in a room and told us to wait a few minutes, and then our names were called. Some were told to report someplace, and I was told I had been put in 4-F because of my allergies, and to go on home.

So I went on back home. I can't say that I was sorry. I probably should have been, but I was kind of relieved because then I knew I could make plans. I thought that the work Julian had lined up was valuable; teaching I've always thought was useful.

I had about a month or so to prepare for my departure. By then, Julian had completed the formalities and had hired as the other IIAA representative to Mexico Donald [D.] Brand. Brand had taken a degree in geography from [Carl] Sauer, and had taken a year's leave of absence from his job at the University of New Mexico. He and I drove down to Mexico City in June of 1944. We went all across Arizona, New Mexico, and Texas to Laredo and then down across the border--the only road into Mexico at that time.

We got to Mexico City, and we registered at the Shirley Courts. That was a motel famous in Mexico among American tourists at the time. As in the U.S., you could drive in and park your car in front of your room. There was also a coffee shop. It was inexpensive, a very nice place to stay.

Brand and I were appointed faculty members of the Escuela Nacional de Antropología, the National School of Anthropology, which was a Master's-level degree-granting institution in the Mexican Ministry of Education. The Escuela was a part of the Instituto Nacional de Antropología y Historia, the National Institute of Anthropology and History. It was located in the old National Museum on the Calle de Moneda, Num. 13, just behind the National Palace, beyond the Zócalo. It was an exciting place at that time.

We were supposed to have an office. We never did while I was there. I had a desk in the office of the director, a man named Daniel F. Rubín de la Borbolla. Brand and I raised a bit of a fuss because in the *convenio* spelling out the rights of each side, an office for American visiting professors was specified, and when we found that no office was available to us, we felt that we were just being given the run around. Later we came to realize that their bureaucracy is far worse than ours. They were doing the best they could.

Moving the Family to Mexico City

Foster: Mickie and the kids then flew down from Berkeley. I had rented a house before Mickie got there. It was quite a deal. It was a big house in the Lomas de Chapultepec. From the center of the city one drove west on the Reforma, with Chapultepec Park and the Castle of Chapultepec on the left, and on up the street, turning to the left to Rio Tajo (Tajo River), a street named after the Tajo River in northern Portugal and western Spain.

Our address was Tajo seis (Number six, River Tajo). And I remember the telephone number: Jota cincuenta y seis zero zero (J 56 00). Mexicans usually answer a telephone by saying "Bueno?" ("Good"), rather than "haló" ("hello"). The caller then normally responds with "Quién habla?" ("Who is talking?"). To be asked by, in most cases strangers, to give my name offended me, so I worked out the idea of answering with the telephone number.

We rented the house from a Mexican diplomat who was stationed in Canada. I dealt with his mother who, because she had broken a hip a short time before, was confined to her bed. In

spite of this handicap she was a vigorous woman, probably not much past sixty, with two phones at hand: an Ericsson on her left and the Mexican instrument on her right!² She was not going to miss any bit of scandal or gossip just because she was laid up for a few weeks.

We liked each other immediately. She said to me, "Now, you're going to need a cook and a maid--no, I think a cook and two maids--the cook full-time, and one maid for the upstairs who in addition to the care of the bedrooms will wash the dog, and one maid for downstairs who will wait the table and wash your car. I know just the people, and I'll get them for you." So she hired them for us.

We moved into this vast barn, which had no central heating. The house had just been finished. The diplomat had never lived in it. First of all, there were bedbugs in the beds the kids slept in. Their furniture had been in storage, and that's one of the problems of storage in Mexico: bedbugs. The kids were bitten before we realized what the problem was.

Oh, I forgot to say that Mickie had decided she'd bring a nursemaid along for the kids, who were then--Jeremy was five, and Melissa was two. So Mickie brought a young girl down with her who knew nothing about Mexico, and it was not a great success, though it made it possible for us to go away occasionally. But she stayed only a few months.

We later hired an excellent person named Heidi Tim. I forget whether she was Viennese or German. Her husband was Swedish, and they had lived in Mexico for some years, so she spoke good Spanish. She was excellent. We were able to leave the kids with her without any fear. She was much more than a nursemaid. We were very lucky there.

To go back to the house, the rains began just about the time we moved in. There was an inch and a half gap under the French doors in the living room, and the rain just flowed in. The roof leaked, also. So we spent the first year getting the house in shape and learning to live in Mexico. We had a big bedroom upstairs where we had an electric heater, which we kept going all winter because the downstairs, in spite of a fireplace in the living room, could not be kept warm. When we ate in the dining

²In Mexico at that time there were two competing telephone companies, the Swedish Ericsson and the Mexican *Teléfonos de México*, Mexican company. Since the two systems were entirely separate, householders had to subscribe to both systems if they wanted to be able to reach all of their friends.

room we bundled up to keep warm. The kitchen was warmer, from the cooking fire and gas stove and oven, but basically the house was very cold. The kids would be outside playing in playsuits in the sun while Mickie and I would be bundled up in sweaters inside. Colds were a problem we faced a lot then.

Riess: Were you in what you would now think of as culture shock at that point?

Foster: I was, indeed. I realized then that one could stay ten years in a foreign country lodging in *pensions*, or hotels, but unless they have to set up a home and hire servants and cope with all the emergencies, enter kids in school, they will never know how the country really works. It was one of the great discoveries I early made, that it's the only way one can ever know what it's really like to be in a country.

Riess: Even though you were living in a fairly palatial structure.

Foster: We were living very palatially, but we had chickens next door. We woke up the first morning in the house, and the roosters were crowing. I thought, "Oh, God, what have we done! We've signed a lease for a year." After that first morning, we never heard them. When we would wake up and hear them it would be kind of comforting.

That was on one side. On the other side there was a house that was being built where there was a night watchman and his family all the time. The houses in Las Lomas were all surrounded by high fences topped with barbed wire, and there were special guards that patrolled all night long, blowing whistles, protecting themselves by announcing their presence so evil-doers knew where not to go. [laughing]

Riess: So much time must have been spent setting up your living arrangements, yet you had to report to work every day.

Foster: I figure I devoted one day in seven to things that in this country I wouldn't have to devote much time to. My usable week was six days, rather than seven days. One incident I remember: after we had been there about three months--we had taken our fridge down with us--our fridge burned out. I had a card from a fellow who called himself the "electroman," in English. He was in the nearby market. He advertised himself as an electrician, capable of fixing things, so I called him.

He came over, and he said, "Well, we've got 50-cycle current in Mexico, and your fridge is for 60 cycles. That's why it's burned out. I'll have to rewire it." He said, "And I'll need an

advance to get the wire." So I gave him thirty or forty dollars, and we put the--I don't remember how we got the fridge down to his place.

Weeks went by and nothing happened. One day I went in to see Pablo Martínez del Río, a Mexican banker and pre-historian, a very sophisticated person who spoke flawless English, English style, carried his handkerchief in his cuff, a real gentleman and a marvelous scholar. I said, "Don Pablo, I've got this terrible problem. I don't know what to do." I described what had happened. He said, "I know you feel that you've been had." He said, "Does the money make that much difference to you?" I said, "No, not really." He said, "I'd advise you to get the refrigerator back and not try to collect."

So I threatened this guy with all kinds of things. [laughs] I was reduced to the indignity of renting a pickup truck and going down and picking up my burned-out refrigerator, which he had done nothing on, taking it home, and we found somebody someplace else who fixed it. I realized then that life is not always fair, and it was a good lesson for me.

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Riess: That capable woman with the two telephones, did she continue to run interference for you in these matters?

Foster: No. I didn't ask her.

We invited them once for dinner, and they invited us once to their house. They had a small apartment--they led an elegant life. When we sat down at the table there was a waiter behind each guest, and there were about six of us, and if you took your hand off the plate, the waiter, with gloved hand, would remove the plate. I had barely gotten started when my main plate disappeared. [laughter] That was interesting, to see how they lived.

We invited them back to our house once. I had made a collection of fire fans, *aventadores*, which are made of palm or tule reeds, and are used to keep braziers--the charcoal fires--burning. I pinned them on the wall of the living room as a decoration. Also we had gone to Puebla and bought the pottery which we still regularly use. And for water and beverages we had glassware from Guadalajara.

I remember the husband, who was a dignified man, picked up one of the plates and turned it over and said, "You'd think after all these years we'd be able to do better than this!" Well, we

had been astonished because their table service had been, we thought, from Sears! Here's all this wonderful Mexican art work for which they had only disdain. But it was very amusing. "You'd think after all these years we could do better than this."

Riess: Was there an eager interest, in Mexico, in what you had to offer?

Foster: Relatively, yes. Mexico was the first Latin American country to become independent in anthropology. They were then building up a big anthropology core. There were a lot of very competent people, high-class scholars. There was Wigberto Jiménez-Moreno, a distinguished anthropologist and historian, rather more than a field anthropologist.

And Alfonso Caso, the archaeologist who excavated Monte Alban, was a superb scientist and a wonderful teacher. Mickie and I audited his lectures on Mexican archaeology. I admired the way at the end of every hour he figuratively tied the bow on the lecture. It just came out perfectly, time-wise.

Something I learned from Caso had to do with students. On one occasion the students were rebelling about something, and when he came to class he found they had taken over his classroom, so he couldn't hold his class in the usual place. He just smiled, and left, and found an empty room where he gave a lecture to a number of students, including Mickie and me. He paid no attention to the rebelling students. I would have been furious. I would have said, "Get the hell out of here!" I learned a lot about patience then. I thought if this man, who had been minister of education, copes with students like this, it's a good lesson for me.

Miguel Covarrubias was another teacher. In addition to being an artist, he was very much interested in pre-history and ethnography. He did a wonderful book on Bali, and he did a similar book called *Mexico South: The Isthmus of Tehuantepec*. He taught in the Escuela, and we became good friends. He was the nicest person in the world. His wife, Rosa, was an American. She was a bit harsh, but--they imagined themselves to be Communists. They weren't Communists, really, at all, but they had been guests of the Communist government in Russia.

Miguel was well known. He was particularly good at caricatures, and he had done a book called *The Prince of Wales and Other Well-Known Americans* that I'll show you. We've got several paintings by Covarrubias.

[tape interruption]

Foster: You ask why Mexicans are interested in archaeology. I think this is because all peoples, when they reach a stage of what I call cultural nationalism, begin to strive to identify themselves as a group, distinct from other groups. They're searching for an ethos. Antiquity--that is, the feeling that their roots go way back in time--is basic to this search for identity. It seems to be a universal craving to know your background, and the further back you can trace your background, and the more elegant you can show your ancestors to have been, the more pride you have.

Mexico is one of the world's lucky countries because of its wonderful archaeology and physical evidence of past greatness: the Mayans and the Aztecs and the Toltecs. The Mexican government has recognized this and has pushed archaeology for a century or more.

One of my favorite stories--this is a little off to the side, but it's amusing. There was a director of the national museum in Mexico named Leopoldo Batres, in the time of Porfirio Diaz, during the first decade of this century. He sold off a lot of the specimens already in the museum for his own benefit. Then, when he no longer had any more to sell, he set up a workshop in Teotihuacan where he manufactured fakes, and he sold those. Finally, he wrote a book denouncing the manufacture of fakes. I thought, "That's a man with real talent!" He knew how to play the system for all it's worth. [laughter]

The calendar stone in the present museum is illustrative of the things that were available a century or more back. Teotihuacan was reconstructed by the time I first went to Mexico in 1936, and Monte Alban had been excavated two or three years before by Caso, so the Mexicans were very much aware of the fact they had this marvelous history.

They had competent archaeologists much earlier than they had social anthropologists. Our friend Matt Stirling, Chief of the Bureau of American Ethnology when I was in Washington, excavated the first "big heads," the baby-faced stone heads of the Olmec culture in Veracruz. Americans have played a major role in Yucatan, too. The Carnegie Foundation subsidized a great deal of research in Yucatan, historical work.

Very little had been done in cultural anthropology. Sol Tax from the University of Chicago had been the first American anthropologist, and his colleague, Norman McQuown, a linguist, had worked in Chiapas with students from the Escuela, I think beginning in 1942. But there had been little regular social anthropology instruction.

I gave a general course, and I asked Borbolla how I should grade. He was very firm. He said, "Grade just exactly the way you do in the United States." I thought, "That's going to be kind of difficult, because these students only come when they feel like it." As for exams--I'd give essay exams--students talked out loud to each other, asking what did they think about this or that. Nothing I could do about it. It was chaos to my way of thinking.

They, on the other hand, were saddled with a guy who spoke very poor Spanish, who was struggling to communicate with them and who was giving facts, many of which were irrelevant to them. But I had some pretty good students, and I had made some friends. I ended up passing everyone who made an effort to fulfill all the requirements, and fifty percent of the class flunked. That was about par for the course. It was an interesting experience.

I realized then another thing that I've been struck with subsequently: students could walk in off the street and register. They didn't have to pay anything, so they could register and go to the courses, and if they dropped out, fine. It taught me that at least a token payment is usually desirable for anything that's worth anything. If it's free, the attitude of people is, It's not worth anything; otherwise, who would be giving it away?

One of the things I've always recommended as a consultant is that at least a token charge be placed on anything that's given to people. Now I think it's definitely agreed that this is basic.

Surveying the Tarascan Country for Field Research Site

Riess: Now what about the field research component?

Foster: The plan was to teach the first semester, and the second semester would be devoted to field research. But before we began the fall semester, Donald Brand and I and Pablo Velasquez, a Tarascan Indian himself, who had been to the United States and spoke pretty good English, made a trip to Michoacan. Don had been over a lot of the area on several occasions--Don was about ten years older than I was, and he knew the country.

We drove to Pátzcuaro, passing through Tzintzuntzan, and went to Ihuatzio and looked it over--Ihuatzio is the town I thought we were going to study. After several days in the lake area we left the car in Pátzcuaro and took the train to Uruapan. There was no highway to Uruapan then. From there we took the train to Apatzingán where we spent a day. That's down in the hot

country. It has subsequently become a very busy town, a good-sized city. A great deal of produce in the United States is imported from Apatzingán. Then it was a small town. Had a little hotel, motel, tiny.

Later we took a bus some distance north, and then we got on a truck, and we spent about two or three days going to a town called Los Reyes, where we spent a night in a mesón. That's a place where muleteers put up with their animals. They'll be given a bed of often just planks, raised planks. We carried our sleeping bags. That was my worst experience with bedbugs. I woke up--I was fatigued, I was so tired I just passed out--I woke up the next morning, and I looked like I was suffering from smallpox, bitten all over by the bedbugs. Fortunately the bites, unlike flea bites, burn like fire for about four hours and then the sensation disappears.

Riess: But you don't know it when you are being bitten?

Foster: Well, if you're sleeping you don't. But you wake up in the middle of the night, and your face or your toes are just burning like fire. I don't know how they got inside the sleeping bag.

From Los Reyes we took horses east across to the volcano, Paricutín, which had come up out of a cornfield the year before. It was a famous site in Mexico. Paricutín itself--there were two villages, and the village itself had been pretty much destroyed, but in the town where we left our horses, and where there was a road going on down to Uruapan, we found the people were still knocking out the wooden doors of the houses and the windows, and the lava was crunching along. It was very slow moving. I could stand fifteen feet away from this ten-foot high mound of molten lava, crusty on the outside, which crunched along as it goes. It was terrifying to see.

Then we went on back to Uruapan and continued by train to Pátzcuaro. So we made a pretty good survey of some of the Tarascan country. We decided that when we came with students in the end of the year, we'd go to Ihuatzio, which is a good-sized agricultural and fishing town opposite Pátzcuaro, on the southern arm of the lake. So we [Brand and Foster] came out to Pátzcuaro with six students from the Escuela--it was after Christmas of '44, about the thirtieth of the month.

The students were Gabriel Ospina from Colombia, Pablo Velasquez from a Tarascan village, Charapan, Remy Bastian from Haiti, and the Mexicans Angelica Castro, Chita de la Calle, and José Corona Núñez--in all eight of us. We put our things in a dugout canoe and ferried across to the town. And nobody would

talk to us. We just couldn't get anywhere. We stayed, I think--I can't remember where we stayed, even, it must have been in the town hall--about three nights. We had formal letters of introduction from the governor in Morelia and everything, but the powerful Catholic priest was very strongly anti-Protestant, and he suspected us of being missionaries.

This was a time when Protestant missionaries were active in Michoacan. I look a lot like a Protestant missionary, I guess--I know I do. I was sometimes confused with a member of the Summer Institute of Linguistics evangelical group named Max Lathrop, a linguist who lived in Erongarícuaro, on the west shore of the lake. He was a man about my age, about the same stature and build, and the same complexion. But I don't think this misidentification was the reason we were not accepted. We were just too many. And the opposition of the priest made it impossible for us to stay.

Rejection and Regrouping

Foster: We were not threatened physically, although I found one of my windshield wipers had been pulled off. There was a road, a brecha, an ungraded dry weather track that connected Ihuatzio with the Quiroga-to-Pátzcuaro highway, so after we had settled in I crossed the lake by canoe again and brought my car into the village.

But we realized that we had to do something. That's when I decided that Ospina and Velasquez and I would go to Tzintzuntzan. And Brand decided that he would take José Corona Núñez with him to make of study of Quiroga, a good-sized Mestizo town five miles north of Tzintzuntzan, on the recently opened Mexico City to Guadalajara highway. Chita de la Calle and Remy Bastiaans worked in Tzintzuntzan with us also for about two months before they went back to Mexico City to go on with the second semester. (In those days, the long vacation in Mexican schools came in the winter, from before Christmas to about the first of March, and students attended classes all summer long, just the reverse of the system in the United States.)

Chita de la Calle was an interesting young woman. She was from a very upper-class family. Her father was a member of the University Club. Before he let her come, he invited me to lunch at the club. He wanted to size me up and see whether he could trust his daughter with me. I guess he decided I was safe. I was always amused at that.

When we got to Tzintzuntzan--we got there the 5th of January (1945)--we went to the *presidente municipal*, that is, the mayor, and talked to him and gave him our documentation. He was a man of only twenty-eight himself, just a bit younger than I. He took us into his home--he had a big home. We had cots and our sleeping bags, so he let us sleep the first three nights in his big living room. He also helped us buy food. I can't remember whether his wife cooked for us or not--it doesn't matter.

Then because this was vacation, and so the school was not in session, it was decided we'd move to the school, where there was running water and flush toilets and showers. The four men started out in the sixth grade, while Chita was assigned to the fourth. Chita and René returned to Mexico City for their classes just as Tzintzuntzan classes resumed, and Ospina, Velásquez and I moved out to the carpentry shop, where we spent about four or five months.

Riess: Was there a priest presence in Tzintzuntzan?

Foster: There was a priest--he was suspicious of me, but Ospina was very good at winning confidence. Pablo Velasquez, the Tarascan, said, "Look, you're going to have to go to Mass every Sunday morning to show that you're not a missionary." So at six o'clock every Sunday morning we were at Mass. Gabriel won the confidence of the priest very quickly, so he was nice and didn't oppose us. He didn't particularly support us, but I think he realized we were doing no harm.

But there were a lot of people who did oppose us. I never knew who they were, but on one occasion, after we had been there about three months, a pot filled with lime was thrown over the top of a wall and hit Gabriel on the head, and broke the pot. Fortunately, his head didn't break. I telephoned Borbolla and told him what was going on. He came out himself to the village, where archaeologists had worked on the *yácatas*, the pre-conquest, so-called pyramids, though they're not really pyramids, on the hill above the village.

He called the mayor, the *presidente*, and said, "I want you to come and bring all your town council." They came with their hats in the hands, and he just chewed them out. He said, "Now, look, these people are here because they're sent by the government. It's your job to help them. They're not going to hurt you." He gave them hell and chewed them up and down. I thought, "Boy, we'll be lucky to get out of here alive; our goose is cooked!" To my astonishment, the air cleared completely. The villagers had not really known why we were there, and even though we made some good friends, there were a lot of people who were

very suspicious of us. Once it was clear that we were there for a purpose, with the support of the government, we had no further trouble.

That was a very good lesson I learned: it's important to let people know exactly what you're doing and why you're there. If we had known that to begin with, we'd have done better.

Riess: Was there prestige associated with having anthropologists in town?

Foster: Not initially, but there's a good deal now. They're the only village in Michoacan with a resident anthropologist, they say. Yes, they're very proud of the fact. We helped develop that. We'd say, "Look, this is not an ordinary village. This village is the first one that the Spaniards came to in all Michoacan, and it's the only one with its own history, *Relación de Michoacan*, written by a priest, about 1540." They were very proud of that.

We explained our presence by saying, "In the United States a lot is known about how city Mexicans live, about the upper classes and how politics work, but nobody knows anything about the village people like you. And we want to be able in our classes to tell students, there's another Mexico, and you are that Mexico." So we emphasized the history, we didn't talk much about culture. But they thought that was fine. I think it was a pretty good approach.

Riess: You say that Julian Steward wanted more "dynamic anthropology." What does "dynamic anthropology" mean here?

Foster: Steward was not thinking so much in terms of real applied anthropology as in terms of using anthropological knowledge about processes of culture change and development to serve as the basis for professional planners. He missed the fact that if an anthropologist is going to have much impact, he or she has got to translate the stuff himself--or herself. (I use "himself"; "herself" was a not a problem of political correctness at that time.)

Riess: This issue of applied anthropology. Were there things you could do in Tzintzuntzan, ways you could engage? Were you tempted?

Foster: No, we were just interested in doing a basic community study. Word pictures of the way of life, the people, all aspects, as many aspects as we could deal with. Brand, of course, did geography. And Julian, being a student of Sauer, also--we were all students of Sauer at Berkeley in those days, so it was perfectly congenial to have a geographer.



Tzintzuntzan, Mexico, spring 1945. George Foster photographed in the plaza. Mickie Foster wearing a Tarascan dress made by Doña Micaela González (pictured on right) with whom the Fosters stayed when in the village from 1959 to their most recent visit in June-July 2000. Doña Micaela died of congestive heart failure on July 4, 2000 at the age of ninety-four twelve hours after the Fosters left to return to Berkeley.

Riess: Now, you were there from January of 1945 to June 1946?

Foster: The first long period was most of the time from January 1945 to the end of August or the beginning of September 1945. Then I went to Mexico City and taught the second semester. The following spring I was writing the book, *Empire's Children*, and Gabriel was still in the village. I'd go out about once a month for three or four days, or as long as I could, up to a week, to fill in gaps in the book as it was being written and to find out what he was learning.

Culture Shock Discussed

Foster: Julian came down in June of '46, the first time he'd come. He wanted to do a little field work himself, so Mickie and I drove him around. My brother, Gene Foster, and his wife, Joan, were visiting us also. We drove down to Oaxaca over the new road which had just been opened, and showed Julian--I guess we took Julian to Tzintzuntzan also. We must have. He was looking for a place to do field work himself.

After we were at Oaxaca he decided he'd fly down to Tehuantepec and look around there. So Mickie and I drove back to Mexico City, driving all night long rather than in the daytime, because our tires were so bad that if we'd driven in the heat of the day they would have all blown out. An all-night trip. The things one does in one's youth!

Riess: Of course, at night you can't see the potholes as well.

Foster: The road was new, there were very few potholes. The advantage is, since there are very few cars, and the lights were on, you can tell if a car is coming around a blind curve. In Colombia Gabriel told me they often prefer to drive at night because of that reason. They can go faster than during the day.

Julian then came back a week later. I asked him what he had been doing. He said he had been sick the whole time. I realized then that he was incapable of doing field work in a foreign country. He was just terrified of the people. He got sick every time he went to the field in a foreign country. He had done excellent field work in Utah and Nevada and the Great Basin, but there he knew he had the county sheriff to back him up, if necessary. His Spanish was not as good as it could have been. He was experiencing culture shock a hundred times worse than I.

Riess: In *Traditional Cultures* you mention the man who wrote first about culture shock.

Foster: Kalervo Oberg.

Riess: Oberg. But he didn't publish about culture shock until 1955?

Foster: He is published only in the Bobbs-Merrill reprint series, about 1955. He developed the idea when he was working for the Institute--well, this does get us ahead a bit.

[tape interruption]

Riess: You saw that Julian Steward was unable to cope with the problems accompanying field work in a foreign country. But you wouldn't have talked about culture shock?

Foster: No. The syndrome had not yet been described, much less named. Oberg was working with the IIAA following the demise of the ISA at the end of 1952. He was in Brazil for several years, where he had started out with the Institute. Among other things he lectured to the new American technical specialists and their families, their wives and older children, just arrived from the United States.

He noticed a repeated pattern of reaction: great euphoria when they arrived, a desire (and an expectation) of doing an outstanding job. Then, after a few months, the American technicians began to realize the facts of this kind of life: they were not going to get everything done they had expected, which in a great many cases led to psychological depression. The lucky ones would begin to regain their sense of perspective, to realize that they had been overly optimistic in their expectations, and that they could still do a good job.

In most cases the technicians recovered from the depths of their depression and went on to do a respectable job. They had experienced what Oberg called "culture shock." I have always thought that this was one of the most important insights about the problems associated with technical aid programs.

Riess: Yet earlier nobody really articulated or studied this?

Foster: If they had, I missed it. It was when one of our students told me, "Why didn't somebody tell me this?" before she cracked up in the course of a very difficult research situation in the Pacific, that I realized how grave the situation was, and how much we could help students by introducing them ourselves to the field when possible, and when not, telling them that they almost certainly would experience culture shock. The field experience was always

held out as the wonderful thing about anthropology--out in this native area, where you live with the people and learn about the way they live. Nobody said anything about how it's pretty tough establishing rapport, and you may feel that you're failing.

Riess: I guess it's a product of selective memory, that after the experience what you remember is the good part.

Foster: Well, that's the good thing about life, I think. We like to remember the good.

Riess: I want to ask you what your recollections are of your own feelings and where you were when you heard about the atomic bomb and the end of the war.

Foster: What I remember best is the death of [Franklin D.] Roosevelt. One of my Smithsonian colleagues, T. Dale Stewart, a physical anthropologist, a biological anthropologist, who died only last year, at the age of ninety-five or so, he and his wife were visiting in Pátzcuaro, staying at the Posada de Don Vasco, a tourist hotel on the outskirts of town. I was having a drink with them at the bar, the radio was on in the background, but I wasn't listening, and all of a sudden I realized they were speaking English. They were talking about this great man, and I listened more carefully and realized that Roosevelt had died. I remember that very well.

I remember when [John F.] Kennedy died, too--I was in a doctor's office. But I can't remember when the bomb went off.

An Upward Move Back to Washington and the ISA

Foster: Going back, when Julian was down in June, he asked me if I'd come to Washington. He said he had a new job. He had decided to go to Columbia University as professor, and he wondered if I would come to Washington and take over the Institute, which I wasn't at all anxious to do. I was enjoying life. The last two years had been a great experience in my life, learning about Mexico, and I liked the life, and I was looking forward to more years there.

But I also realized that as a young anthropologist it was wise to take advantage of any upward opening. I don't know why I felt that, but I did. I think my advice to myself was good advice. So after Julian went back to Washington, Mickie and I flew up to Washington, and we bought a house at that time. Then we flew back to Mexico and closed things up. It must have been

about July. She and the kids flew back. I drove, with Gabriel Ospina, from Pátzcuaro--the last time I was at the village--to Guadalajara and spent a day or so. And there I said goodbye to him.

Then I drove across to San Luis Potosi and back to Laredo and then to Berkeley to visit Mickie's parents. By the end of the summer we were settling into our new house in Washington, and I was learning the job. I expected to be there indefinitely, though I didn't expect to be there forever. But it might have worked out the other way.

I remember the beginning of the war so clearly. It's odd that I can't remember the atom bomb or the surrender.

Riess: Why did the interest in Latin America disappear after the war?

Foster: I think South Asia and China and Africa were so much more volatile. The war had made the United States aware of the fact that trouble in far-off countries could influence our lives here, and South America seemed relatively calm. With limited money, the government was putting its funds into Asia and Africa.

Riess: What was your assignment as the director of the ISA? What did you expect to be able to do?

Foster: Well, one, I thought it would go on indefinitely, being young enough not to have much sense of time depth of my own. When I realized having to get a budget approved every year was kind of chancy, and when I began realizing the problems of getting security clearance for people--. This was just before [Senator Joseph] McCarthy's downfall. The government was very, very cautious about--well, at first they were not. From '46 to about '50 I had a free hand in hiring people. It never occurred to me that I'd have to tell them, "You have to be cleared." After that, that's when it began being tough.

Riess: In writing about that time you say that by 1950 it was hard to get money for "cultural relations." What are "cultural relations"?

Foster: Exchange professors and USIS [United States Information Service], libraries. For example, the Benjamin Franklin Library in Mexico City was set up by the United States government. That was a great success. [laughs] People could take books out of the library legally instead of having to steal them, which they did with books from their own libraries.

[Interview 5: January 6, 1999] ##

Foster: [speaking of Tzintzuntzan, where the Fosters spent their 1998 Christmas holidays]. Four years have gone by since we were last there at Christmas, and a half century of research, and the changes continue to be striking.

Mexico, of course, has had very tough economic times during these years. Were it not for all of the young people and not-so-young who live in the United States and send money back, the village would really be suffering. These last three years they've had a remarkable mayor, who is the only mayor I've heard complimented while in office. Usually, anyone who wants to seek office by definition is an undesirable character. This young man is a Tarascan, from Ichupio, trained in medicine. I know his mother and several other close relatives, but I have never met him, probably because he was away doing his pre-med studies and then studying medicine. He has paved all of the streets, he's put in new lighting, he's dug a new well--he's done everything the village needs. It's just remarkable how clean it is.

We got there on Sunday, the 20th of December, 1998, in time for the fifth *posada* of the Christmas period. Each section of town puts on a *posada*, which is a reenactment of Joseph and Mary going through Bethlehem, asking for shelter. There's a small float of Mary on the donkey, and Joseph, standing, about eighteen inches high, carried by four young girls who are from that section of town. Behind them come people carrying big, lighted tapers and at the rear, a musical band. They stop and ask for *posada*, but the request is rejected. Finally the float comes to one house where they're accepted, and then they go inside and they're fed.

The interesting thing about a *posada*--there are many interesting things--is the fact that the *piñatas* are broken after *posada* has been granted. *Posada* means a place to spend the night. The government inns in Spain are called *posadas*, for example. There's one in Pátzcuaro, the *Posada de Don Vasco*. So *posada* means a place where you can spend the night. Granting *posada* means--Mary and Joseph were granted *posada*, they were given a place to spend the night, in this case in the manger.

After the procession is inside safely, the *piñatas* are broken. That gets a little dicey. Boys get a bit out of control, and they throw firecrackers, *buscapies*, "look for the feet," little fireworks that scamper along the ground. I've never known anybody to be injured, but I don't know why. And the rockets are shot off. It's quite a show.

The Christmas *aguinaldo* is part of the *posada*. The people who are in charge for this particular evening bring gunny sacks full of small paper bags that may have a mandarin orange and a piece of sugar cane and tiny little candy bars and hard candy, which they give to everyone in the street--adults as well as kids. And then they have the *posada*, so it's a lot of excitement.

What particularly interested me this time was that every house on the street had a Christmas tree outside, with flashing lights. Many of the trees had a kind of a whistle that played "Jingle Bells" and "Silent Night." And the street was literally scrubbed. I saw them--when I got there, housewives were scrubbing the street and the sidewalks with soap and water. I've never seen the town so shiny.

Across every street, high up, about twenty feet up in the air, are strings of what is called *papel chino*, crepe paper that's cut in fancy designs. You've seen it in all kinds of Mexican fiestas. And inflated balloons. So the atmosphere is incredibly festive.

Riess: Is it a town that attracts tourists? Is this done for tourists?

Foster: No, this is done for the people. The village does attract tourists for Easter and for All Saints and, to a lesser extent, for Corpus Christi. But basically this is for the village, itself.

Riess: Does it mean that fewer people are leaving the village?

Foster: No, more and more are leaving the village. But they send money back, and they come back for Christmas. They drive back, or they now fly back.

The increase in plane service between this part of Mexico and the United States, especially California, is incredible. One can now get a nonstop flight from San Francisco to Morelia, and there are three or four nonstops a day to Guadalajara, which is a flight of about four hours. For a long time the young men who would go to El Norte, the United States, would take a long bus ride, a sixty-hour bus ride. Now they've learned that time is money, and they much prefer to fly. So every flight to Mexico from the Bay Area, particularly at Christmas, has all kinds of legals--legal residents--and some illegals as well, I expect.

Riess: You are saying that these changes were surprising.

Foster: They've been coming for several years, but this is the first Christmas we've been there in four years. I hadn't expected quite so much activity.

It's a town that has always had a very rich ceremonial life. I've wondered if it could be a question of continuity from pre-Conquest times, when it was ceremonially the most important community in this whole area. Ceremonial life is the one activity in which the town always will cooperate, which is not to say they don't have arguments, but they'd feel very badly if they let the village down and didn't fully fulfill their obligations on all of these fiesta dates, of which there are so many.

Riess: How were you welcomed? Has your welcome from the village changed over the years?

Foster: I'm less well known than I was twenty years ago. That's very interesting. In 1978, Mickie and I were driving through Yucatan, the only time we've been in Yucatan, and we stopped in Chan Kom, where Robert Redfield and Alfonso Villa Rojas had done work in the early 1930s.

Villa Rojas was there at the time, doing a re-study, and we spent an afternoon with him. He took us around the village. I thought to myself, "My God, Alfonso has lost touch with this village. He doesn't know who the people are. He doesn't really know what's going on." I was rather saddened. Well, I know what it's like, because if I were in Tzintzuntzan on my own now I'd have much less knowledge of the village than I had twenty years ago. At that time I was working more intensely, I was taking a census every ten years and keeping the vital statistics up, so I knew many more people. I turned that over to Van Kemper, so now I don't even have the vital statistics in my house here in Berkeley.

Riess: Van Kemper is one of your students?

Foster: He's a former student. He's a professor at Southern Methodist now. Along with Stanley Brandes, he's keeping in touch with the village.

A whole new generation or two has grown up since I was there doing intensive work. Not only do I not know them, but a lot of them don't know me. At one time, I think everyone in the village at least had an idea who I was, but now I've asked people--I say, "Do you know me?" And they say, "No, I don't." I'll ask them who their father and mothers were, and they'll tell me. But they're surprised when I say, "Yes, I knew them." And I'm surprised that they haven't at least heard of me. A lot of them simply tell me

they've heard of a gringo who stays in Doña Mica's [short name for Micaela] house, who's said to be an anthropologist.

Riess: Is that something you're curious about, how you've become part of their collective past?

Foster: Yes. I go to the village. I reach a high point of recognition, and then I'm passed by, if I live long enough. As I say, I'm much less well known among the villagers now than I was twenty years ago, which I find quite fascinating.

Riess: What are other ways that you can measure change, other than the cleanliness of the town and the kind of energy?

Foster: A tremendous amount of building of new houses and improving old houses. Almost all with money from the United States. Many of the houses will never be lived in by the people who have built them, because although they think of retiring there, their children have gone to school in this country, or they've married in this country, and the parents are rarely inclined to go back and live alone in Tzintzuntzan. They prefer to live in this country. The houses--some of them stand or stood empty for years, or have somebody who is a caretaker who lives there for nothing.

Riess: Is there a tradition of the old, abandoned pueblo, that people go back to visit ceremonially, like Acoma, in New Mexico?

Foster: No, there are no abandoned villages there. You find them in Spain, Stanley Brandes tells me, but not in the Mexico that I know. So many kids are coming along--even though there's a constant outflow, nature is bringing in an equal number, so the village--well, at Christmas time you can't tell because there are so many people back, several hundred, at least, and they're out in the streets, so it gives the impression of a very full village. I'd like to get down sometime when I'm not at a fiesta and see. I think it would be much more quiet, the life at that time.

People complain about hardship, economic hardship. I think a lot of them are having difficult times. Medicine and medical care have become much more expensive. Although schoolteachers and their parents, and some others as well, have Social Security, the ordinary villagers don't have Social Security, and yet they've become accustomed to medical care. Medical care is a very significant item in their budgets.

Riess: I should think that would be one the hardest parts of your long-term relationship, seeing that.

Foster: We have spent more money on medical care for friends than we have on anything else for the village over the years. This last year, I suppose I spent a couple of thousand dollars just in medical help for these people. Initially, what a hundred dollars will buy today could be bought for five or ten dollars, so in a sense it was not a big item, but it gets to be more and more of an expense for me.

Riess: When you see that money can solve a problem, is there any compunction--what is your take on that?

Foster: I just play it by ear. Some people I've helped for years, other people I've helped very little, most I haven't helped at all. It depends on the situation and how well I know them and how I size up their need.

It's hard on my students and their students because I've been able to help more than most anthropologists, so the villagers tend to get the idea that all anthropologists are well-to-do and can help if they are approached in the right way. So whenever I have an opportunity, I point out to people that the students are not able to help them, and they shouldn't expect the support that older anthropologists are able to give them.

Riess: Have they ever had a Peace Corps representative there?

Foster: No, I don't think Mexico has ever had a Peace Corps program. The closest thing they've had is the CREFAL program that my Colombian colleague and former student, Gabriel Ospina, worked with in Pátzcuaro. I've written about that in *Tzintzuntzan: Mexican Peasants in a Changing World* (1967). It was a community development effort in the village that, like most formal community development programs at the time, had very little impact. But there's never been a Peace Corps program.

VI SPAIN, 1949, AND RETURN TO CHANGES IN WASHINGTON, 1951

Thinking about Spanish Influence in the New World

Riess: To turn to the work you did in Spain. What is the background of that?

Foster: I came back to Washington in 1946, in July or August, to take over the Institute of Social Anthropology in the Smithsonian Institution. I have talked about that. For a couple of years I was busy developing my knowledge of how the program worked and with developing the Latin American program, particularly in South America. I made my first trip to South America in February of '47, and again in February of '48. The second time, Mickie went with me. We were gone for two months. We had a wonderful swing all around South America.

I realized that Mexico is not the only part of the New World where the Spanish influence was tremendously great. Now, this was a period in the development of American anthropology when a major theoretical interest had to do with acculturation, that is, what happens when two or more societies come in contact with each another: how do they change, and what are the forces that change them? Herskovitz, who had been my major anthropology professor as an undergraduate, was one of the pioneers in this field, as were Robert Redfield and Ralph Linton. I knew all three of them.

I was very much concerned with acculturation, and I realized that in order to understand what was Spanish and what was not Spanish in the New World, we had to get data on Spain. I had mulled this over ever since coming back from Mexico, waiting for the opportune time. I seriously got down to work in the late summer of '48, when I decided I would go to Spain for a year and do a study similar to the one I did at Tzintzuntzan.

At that time, American anthropologists believed that most of the emigrants to the New World came from Andalucía or Extremadura,

southern or western Spain. So I thought, "I'll pick out a village in one of these two major areas, and we'll stay there a year and get all the data we need for a comparative study."

Riess: Very neat.

Foster: Yes. It sounded just great. The question was--the Spanish Civil War had ended in '39, just at the beginning of World War II. Spain was a pariah, and there was a question as to whether an anthropologist would be allowed to even work in the country. I thought a lot about it, and finally decided to go to the Spanish Embassy and tell them what I'd like to do and see what I could arrange.

Organizing the Trip, Contacts

Foster: I called and made an appointment with the cultural relations officer, who was a remarkable man, named Pablo Merry-del-Val, a tall, handsome man, not much older than I was. I explained to him--I said, "I'm not in the least political. I'm aware of the history, the civil war, but I can assure you honestly that I have no political agenda. What I want to do is study the historical background of the present popular culture."

He was very receptive. He said, "I'll help you." He said, "It would be very helpful if you could go to Spain immediately and meet the right people. Can you go?" I thought, "Well, I guess I can." So two weeks later, I was on a TWA Constellation going to Madrid.

Previously I had talked with Louis Hanke, the historian who was head of the Hispanic Foundation in the Library of Congress. He was a marvelous fellow, a wonderful historian. He put me in touch with an equally interesting and wonderful woman named Marie Cannon, who was a cultural relations officer in the American Embassy in Madrid. She was a sort of godmother to all the scholars who came to Spain.

I flew to New York. In those days I don't think Idlewild Airport, which much later became JFK, had even been thought of. I left from La Guardia Airport on a TWA Constellation, a four-motored, pressurized Lockheed plane, the last word in luxury transportation. It carried about fifty passengers. We flew to Boston and stopped to pick up passengers. Then we got back on the plane after about an hour and sat and sat and sat. Finally, the stewardesses served us our dinner on the plane, on the ground.

Then they said, "Sorry, but we're going to have to put you up at the Parker House tonight. We can't start one of the motors."

So they bused us into town, and I called Mickie, and I said, "This is a wonderful flight. I'm calling on the new air-to-ground telephone." I said, "Here we are at sixteen thousand feet above the ocean." She said, "You're drunk. Where are you?" I laughed and replied, "I'm in the Parker House."

So we left the next morning. There were only about six passengers on the flight, so instead of going to Gander we cut straight to Santa Maria in the Azores, which during the period before the jets had a very busy airport. It had been developed during the war, and the islanders sold wonderful lace tablecloths and blouses, napkins, handkerchiefs. It was just as busy as could be, with all kinds of planes coming in in the middle of the night. Then we went on the next day to Lisbon, and finally to Madrid, where Marie Cannon met me at the airport. We got along fine. We were good friends for many years.

I was put up at the guest house of the Consejo Superior de Investigaciones Científicas, the Superior Council of Scientific Investigations. I was introduced to a number of people. Pablo, with whom I'd gotten on a first-name basis, had told me, "My mother lives at the Ritz Hotel. She'd like to have a reception for you." By then I caught on to the fact that his father had been Ambassador to the Court of St. James for many years. He was a member of one of the top families in the country.

His mother was a *grande dame* who lived in the most elegant of the hotels in Madrid. She had a reception that was unlike anything I had ever seen. The officials of the museums and the Consejo were all there. I realized immediately that symbolically she was telling these people, "This man is okay, you can trust him, he's got my son's stamp of approval." It was a very generous gesture. (Incidentally, Pablo became the very popular ambassador to the United States twenty-five years later.)

I was amazed, too, that everyone addressed me as *tu*, because in Mexico the last thing you do is address a stranger as *tu*. That, again, I took as an indication of acceptance, that they had brought me into this system.

Riess: And that they decided you were in the same social class.

Foster: Well, or at least aspiring to it.

Riess: Was that also true in Latin America--that you had to be accepted at the highest level in order to function at a lower level?

Foster: No, it was quite different. In any country, of course, the higher your level of acceptance is, the easier it is to work. But when I did my work among the Popoloca, my doctoral dissertation, I knew the people in anthropology, I had become acquainted with them, but I had no political contact at all. So this was a new, different experience.

Riess: Did you tell them what the questions were in your own mind? The questions of culture and conquest? When you met with people, did you tell them what you were actually hoping to find?

Foster: I told them that one of the questions anthropologists and historians working in the New World had was, "What is the point of origin of this trait?" Or, "Does this trait represent a Spanish contribution to Spanish American cultures, or is it indigenous to the New World?" I explained that by studying Spanish popular culture I'd be able to unravel a number of these questions. I had no theory as yet, other than the certainty that I'd find in Spain many of the culture traits that I already knew in the New World.

As matters turned out it wasn't that simple. I did, of course, find many traits, but often in a different form than I had expected. For example, I knew that the grid plan town found in every Latin American country (the checkerboard pattern of the U.S. Midwest) had come from Spain. So I was astonished when I observed Spanish villages and towns and found they did not conform at all to that pattern. But that's another story I'll tell you a little later.

With respect to Spanish American popular medicine, I assumed the belief that foods and medicinal herbs and other substances have a metaphorical quality of "hot" or "cold"--nothing to do with thermal temperature--would characterize Spanish popular medicine. I was dumbfounded when I discovered this to be absent in Spain.

While there, I became acquainted with Julio Caro Baroja, a very interesting person. He came from an intellectual family. The Baroja family, his mother's family, was the more prominent side of the family. His uncle, Pio Baroja, was one of Spain's principal novelists. His uncle, Ricardo, was an artist of no mean ability. Julio hated the Franco government like everything. His father--he was a printer, and he had run-ins with the government, and Julio blamed the government for his death. He was not killed, but I think his health was damaged.

Julio had an immense--he was the anthropologist who controlled the literature--. But I'm getting ahead of myself. While in Spain I also flew down to Seville and met some of the anthropologists and historians there. After two weeks in Spain I

returned to New York. That was a grueling flight, on a TWA DC-4, a non-pressured four-motored plane that cruised at about 150 miles an hour. The scheduled time was twenty-eight hours from Madrid to New York City, and it arrived on time, if you can believe it. My legs felt like lead when I got to New York City.

The Consejo Superior was Franco's major scientific organization, established by him in 1939 at the end of the civil war. And in 1949 it was having its ten-year jubilee--kind of rushing things. The Smithsonian received an invitation to send someone, and I thought, Well, I'm the ideal person to go. So in the spring of '49, Mickie and I went to Spain for two months, and I represented the Smithsonian at the ceremony. We took the rest of the time traveling around the country, looking at various areas to decide where we'd like to do our work.

Touring Spain with Mickie, Spring 1949

Foster: In those days, it was customary for tourists to take a car with them, so we took a Plymouth sedan with us on the boat. It cost a hundred dollars to take the car, it wasn't very expensive. We landed in Gibraltar. We had two wonderful months.

But we changed our research plans, for two reasons: one, the Spanish villages were isolated, and they were dirty. I just couldn't see taking small kids into them. Also, by then I had been doing enough research to realize that the influences in the New World had come from all parts of the country, that the *conquistadores* were not all from Andalucía and Extremadura, that if I took any one part of the country, I'd get a very warped view.

I had become much better acquainted with Julio Caro. During the time I was there we worked out an arrangement whereby I'd come back in the fall of '49, and Mickie and I and the kids would take a house in Madrid for a year. Julio and I, in my car, would make trips to various parts of the country to observe fiestas and study the material culture such as house types, agricultural tools, and popular arts and crafts such as pottery. Prior to the trip he would call my attention to the most significant publications on the ground we proposed to cover, so that I could read up on the areas in advance.

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Riess: It's interesting to keep in mind that you were not an academic, and yet you were on an academic undertaking.

Foster: What do you mean I wasn't an academic?

Riess: Well, I mean, you weren't at Berkeley. [laughs] It's a very narrow view.

Foster: I'd say it is! [laughter] I certainly thought of myself as academic. The Smithsonian Institution was older than the University of California by a generation or so. My academic credentials were thoroughly praise-worthy.

Riess: Yes, I know they were.

You were doing quite an original piece of research, this study of the conquest culture?

Foster: I think it probably was, although it was not unique. But it was, I think, a major step, yes, I do.

Riess: Why? Why weren't people studying the sources of the Italians who came into the New World or the Spanish or the Africans?

Foster: Acculturation theory was something that developed just prior to the war. During the war, of course, it was impossible to do that kind of work. After the war, there were still very few anthropologists in this country, compared to now. I was one of the first to become interested in this type of research, in part I think because of having been Herskovitz's student. Then through my interest in Mexico, being aware of Redfield's work and his concern, it just seemed to me like the next logical thing to do.

Certainly it was very profitable for me, intellectually, and I hope for other people.

Riess: So you settled into a pattern of library research?

Foster: And trips. Julio was doing his research continually. He was a fabulous writer. We'd plan to go to a particular part of the country, and he'd call my attention to the ethnographic literature, which he knew like the back of his hand. I had to go through it--there were no Xerox machines at that time and I hired a secretary who worked full-time, typing notes, if you can believe it. Spain was incredibly inexpensive at that time. I don't suppose I paid a secretary more than a dollar and a half a day or so. It was not a great expense, but it was a terrible nuisance. When I think of how easy it is to do that kind of research today I weep, to think of all the effort of typing article after article.

Then we'd drive to the area. We went to areas that Julio had not seen himself, so it was a great advantage to him. I think

I got more out of it than he did, but I'm not sure, because he has a whole book that deals with our travels in Andalucía, and even includes a few of my photographs--I'll show you the book in a minute.¹

In any event, we would start out, and we'd go, let's say, to Valencia, and then we'd go down the coast a way. He knew different villages which had been in the literature, so we'd go to them. I learned about Spanish windmills and irrigation systems on that trip, which are not very important in Spanish America, in Mexico at least.

Then we went to a series of fiestas in Huelva, southwestern province, where--well, I just can't begin to describe them--I should have read up if I had known I was going to talk about this. In any event, we worked for the whole year.

An interesting thing was that in the spring, about March or April, my parents flew over to spend a couple of weeks and travel around. My mother was sitting next to a very pleasant young woman who was from Berkeley. She said, "There's only one person I know in all of Spain. I hope to see him." My mother said, "What's his name?" And the young woman said, "George Foster." [chuckling] That was Eugenie Carneiro, who was a fixture in the I House, where I lived when I was first a graduate student. She was a much loved person by a great many generations of I [International] House residents. We had the pleasure of traveling a bit with Eugenie and taking her to dinner several times, in addition to my folks. Small world.

Riess: When you were traveling with Julio Caro Baroja, where did you stay, how long in the villages? How did you behave?

Foster: We stayed for up to a week.

We'd introduce ourselves--. The interesting thing about Spanish villages is there was almost always a doctor, or somebody who's better educated than in the kinds of villages we studied in Mexico. Caro Baroja was a name--the Baroja was recognized because of his uncle, Pio. So Julio had a ready-made entree because his name was recognized.

The Spaniards are terribly welcoming. This is a thing that interested me tremendously. In the spring, when Mickie and I were traveling alone, we would go into a village, and stop opposite the

¹ Julio Caro Baroja, *De etnología Andaluza*. Malaga: Servicio de Publicaciones, Diputación de Málaga, posthumously published in 1993.

church, and almost immediately somebody would come out and introduce himself, mostly likely the doctor, but it might be the mayor. He'd say, "Come and have a copita with me," and take us into a bar, and give us a bit of sherry. They were very welcoming.

I remember in one of the cities in Extremadura we were out walking at dusk one evening, and a well dressed woman came out of her house and asked us if we were British. We said, "No, we're American." She said, "Won't you come into my house?" We were delighted to. It turned out that she was a friend of the Merry-del-Vals, so we were able to establish a contact there. That's illustrative of the way they are. They're so welcoming, at all levels.

We were in a town called Montánchez for Holy Thursday, Maundy Thursday in '49. The men took me with them. And the dames of the city were sitting on a balcony with Mickie. I found myself in the procession with a lighted taper, walking along, looking solemn, pious. Mickie nearly choked when she saw her husband coming along. Interesting to be accepted like that. We never had the least bit of trouble. I think they believed us when we said we were interested in popular culture and the history of the community.

The most remarkable place was a town called Bujalance, near Córdoba, where Julio and I spent most of Easter week the following year. That was a real bash. The upper-class men belong to sodalities that are devoted to a particular saint. During Easter Week the members rent a hall and hire a chef who cooks all of their meals during these holy days. The sodalities bottle their own wine, complete with labels carrying the name of the sodality. Much of every day and night is spent carrying enormous floats with images of Christ, the Virgin Mary, and other biblical figures. The processions are very similar to those of Seville and Córdoba. It's a curious combination of holiness and debauchery.

Julio and I were adopted by one of the groups, so we went to their meetings. I never carried a float, but I could take pictures. I had the cooperation--they'd stop the procession any time I wanted. I was in the middle of a procession which would go on both sides of me, so I got fantastic pictures. The last night, after it was all over, I was astonished--and I think Julio was, too--they said, "Well, let's go visit the muchachas now, shall we?" In other words, "Let's go visit the whores." This was Saturday night. I thought, that's an interesting combination of piety and debauchery. That was a wonderful year.

Riess: Do you find those social structures and behaviors in Mexico?

Foster: No, in Mexico it was quite different, for reasons you can understand. The Indian and *mestizo* villages have been exploited all through history, and any stranger who comes to town generally means trouble. In Spain--this is not to say that there weren't problems in the Spanish villages, but the history was different. The Spaniard is very different in personality than the Mexican *mestizo*. I think the Spaniards have a genuine sense of obligation, of hospitality, that I had rarely seen in Latin America.

Some More Thoughts on Conquest Culture, Grid Plans

Riess: In your foreword to *Culture and Conquest* (1960) you say you found no easy solution to the problem of relating the data to Spanish America, that you didn't find a one-to-one relationship.

Foster: The reality was much more complex than I had anticipated, which is not surprising. I had a simplistic view of the way societies function. I had assumed that all culture traits that reached America from Spain followed a similar pattern of diffusion. The theoretical ideas in the book are the outcome of the work. I didn't have any of them when I started out.

The first major division I made was that between the elite culture and the popular culture. I realized that a great deal of the influence has been at a planned level. For example, popular medicine in Mexico is quite different from most of the popular medicine in Spain. The question was how do we find Greek medicine at the popular level in Mexico and Latin America, and not in Spain?

The conclusion I came to--and I am convinced that it is the correct one--was that at the time that Greek medicine was introduced into Spain by the Moors, beginning in the eighth century, and particularly in the ninth, tenth and eleventh centuries, there was already a vigorous popular medicine in Spain. And the regional Spanish cultures, although they were vastly changed by the Moors, were not shattered the way they were in the New World. Greek humoral medicine remained at a sophisticated level in all of Europe, and in the United States too: at the time of our independence, medicine was largely Greek. Benjamin Rush, one of the signers of the Declaration of Independence, bled his patients dead during the yellow fever epidemic of 1793 in Philadelphia. Purely humoral medicine.

In any event, I realized that it had come to America at the level of the friars and the medical schools, and then it had filtered down, as does so much popular culture.

The same with the grid-plan town in Latin America. I was astonished when I didn't find grid-plans in Spain. When I began reading, I realized that this was an idea the Spaniards had developed about the time of the Conquest of the New World. They realized it was easier to keep track of people and to rule a community if it were laid out in an orderly fashion, with square blocks surrounding a central plaza. There were very elaborate regulations drawn up specifying the length versus the width of a plaza, and where the streets would come in. That was one of the most interesting things I encountered in Spain.

That taught me something else about writing. That is, when you get a good idea, it's best not to bury it as a chapter in a book, because if it's mixed in with a lot of other things it's going to be overlooked more than if it stands out on its own in a major journal. I've always been sorry that I didn't write my findings on the origin of the grid-plan town in Latin America as a separate article rather than as Chapter IV in *Culture and Conquest*.

I concluded--and I think the evidence bears me out--that the idea for the grid-plan town in Spain stems from the "new towns," the *villeneuve*s and the *bastides* of Aquitaine in southern France. The *bastide* was not a fortified town, but a town on a semi-grid-plan that developed along the route (the *camino*) of Santiago de Compostela. Influenced by pilgrims who came from or through Aquitaine, the plan of towns along the *camino* in northern Spain began to foretell the grid-plan--not a true grid-plan, but in old maps one can find the germ of the concept.

Then in 1492, when Ferdinand and Isabella decided the time had come to drive the last Muslims from Spain, they converted a military camp, which was laid out according to a grid, into a permanent city, Santa Fe, within sight of Granada. The tents of the camp were replaced by stone buildings, to show the Moors in Granada that the Spaniards had come to stay. That was the first real grid-plan in Spain, a military plan. In the eighteenth century there were German settlements in Jaén and other south-central parts of Spain, where grid-plan towns were built, too.

But the origin in the New World turns out to have been, as with medicine, an elite plan. The government decided it wanted new communities built according to the grid-plan, for the reasons just given, and so they were.

Riess: That's interesting.

You brought up the issue of publication. You didn't publish *Culture and Conquest* until maybe ten years after your work there.

Foster: In 1960, yes.

The history of *Culture and Conquest* is interesting: I wrote a much longer version, which I submitted to the University of California Press, which turned it down. I've always resented it. I think the Press was right in rejecting the manuscript, but I thought, and continue to think, that if I had been told why it was being rejected, and given positive criticism, I could have done an acceptable job.

Sherry Washburn was the savior there; he accounts for the fact that the Wenner-Gren Foundation brought it out. Following his criticism I re-thought the thing, and I realized that it was too factual, it was not theoretical enough. I developed new theoretical ideas that went into the monograph, and I think it's been a fairly influential volume, still cited quite often. A new Spanish translation is planned by a commercial press in Sevilla. The only Spanish translation (1962) has been criticized as badly done. I think it interesting that in Spain it is felt that a revised translation will sell enough to justify the expense.

Riess: Developing theory from fact sounds like an intellectual push.

Foster: Well, I see why I emphasized facts so much. First of all, I am factual in my research, I always work from real data. There were so many data that I felt ought to be put on the record that I failed to recognize that many readers would be bored to tears. It was only when Sherry gave me his sage advice that I realized I could cut out a lot of the fact and still leave enough so I'd get the message across, and in doing that, some of the ideas became clearer.

The idea of what I call a "conquest culture," for example. That is, that the total face of Spanish culture was never presented to the New World. Spanish culture had gone through a filter before leaving the country, and was filtered again by the perception of the people who received it. That is, not all items of Spanish culture were made available to the New World, and for a variety of reasons many of those that were never took root in Americas. I think that was a very interesting idea.

Edward [H.] Spicer, one of the great American anthropologists who worked on Mexican and Mexican-American

culture, made use of the idea. It has not been widely used, but I don't think there was anything in the concept that caused dissension among scholars, at least at the time I first advanced it. Subsequently my view on the origin of humoral medicine in the New World has been the most contentious thing I've ever written. There are several Mexicans, and one or two Americans, who think that it's Aztec, but the evidence for Spanish origin is overwhelming. And I think most people agree with me.

Riess: It must be fun when things become contentious?

Foster: Well, I sometimes give as good as I can take, I hope.

Riess: So that phrase, those two words, "conquest culture"--that's yours?

Foster: That's my construct, yes.

Riess: Writing the book took place here at Berkeley?

Foster: Yes, most of it. I began in Washington, but mostly it was written in Berkeley.

Riess: Having stepped awkwardly into the academic question earlier--I wonder whether the collegial surroundings that you had in Berkeley were--were you talking about ideas more here at Berkeley than you would have been at the Smithsonian context?

Foster: Yes. The university was more stimulating than the Smithsonian, although at the time I was at the Smithsonian it was very good. Gordon Willey was there, one of the great archaeologists of this century, later at Harvard, now emeritus. He ran the Institute for me the year I was in Spain. Although an archaeologist, he was very good to talk to. And Phil Drucker, who had taken his degree with Kroeber and worked with Heizer, and Matt Stirling--both of them were very good to talk to. But there's no doubt the University of California was a more stimulating environment for a writer and a scientist than the Smithsonian at that time.

[tape interruption]

Riess: Could you have done this work the other way around? Studied Spain first and then gone to Mexico?

Foster: Generally, I think, in acculturation studies, anthropologists have taken the--there's almost always, perhaps always, a dominant and a subordinate culture. One that's stronger that has the power to impose on the weaker. I think anthropologists, by the nature of their research, have probably always had a primary concern with a subordinate culture, the one that's most changed. I'm sure that

if I had started out in Spain I would have followed up all kinds of other interesting things, but I wouldn't have come up with anything like a "culture of conquest".

Riess: Thank you for letting me read the excellent journal of your travels in Spain. But I know you didn't keep journals.

Foster: No, as I've said, my great scientific regret is that no one ever told me the importance of keeping a journal. I wrote that about three or four years ago [1995].

Riess: It's very fresh in its feeling.

Foster: I have letters that I sent to my parents, many of which they saved after passing them around--a lot of them they didn't. And my line-a-day, and things like that. But it's a pure reconstruction.

Moving ISA Anthropologists into Public Health

Riess: After you came back from Spain, you returned to your work in Washington, and the next major area you entered was medical anthropology.

Foster: I'll tell you how I got interested in that. Applied anthropology was a field that got started shortly before the war. The Society for Applied Anthropology was established in 1941 or '2. It published a quarterly called *Applied Anthropology* until about 1950 or '52, when the name was changed to *Human Organization*, which is the name it has to this day.

Riess: *Human Organization*? Was there any ruckus about changing the name?

Foster: I don't know. I wasn't a member of the society when it was changed. I'd had nothing but disdain for applied work. Herskovitz couldn't stand it, and Kroeber and Lowie didn't like it either, so I just thought I'd never touch applied anthropology.

Well, when I got back from Spain I realized that the funding for the Institute for Social Anthropology, which had been on State Department annual grants, transfers of funds, was about to run out. I had the problem of either closing the Institute up right away, or looking for money elsewhere. As I've said, there's nothing like the threat of financial disaster to make a person re-examine his fundamental premises. [laughter]

It must have been in the spring of '51 when I decided we'd have to demonstrate that we could tie in with what was then known as the Point Four program. It was first--it was the Institute of Inter-American Affairs, which was the original American technical aid program, only on Latin America at that time. And it was about to be expanded to become world-wide. It was known as Point Four because it was the fourth point of Harry Truman's inaugural address. It has had a series of different titles: at one point, it was the International Cooperation Administration; at another point, it was the Foreign Operations Administration. But at this time it was still the Institute of Inter-American Affairs.

I felt that was the possibility we had. I knew that this program, which I had worked for briefly in 1943, when I came back to Washington from UCLA, had three main divisions. One was health, one was education, one was agriculture.

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Riess: You were saying that agriculture was all tied up by the rural sociologists.

Foster: Yes, and education, the anthropologists didn't have much on education in Latin America that would do anyone any good. So that left health. I thought, Now there we *do* have something to offer. Every anthropologist has gathered data on popular medical beliefs and practices, and that will have a lot of bearing on the acceptance or rejection of modern medicine. So I went over to the Institute, which was almost across the Mall from the Smithsonian at that time, and I talked with the acting head of the health division.

He was not awfully receptive, but I said, "We have anthropologists in Mexico, in Guatemala, in Colombia, in Peru and in Brazil who know a lot about the actual health behavior of people. If you'll clear it, I'd like to ask them to get in touch with your local heads of party and find out what the problems are and gather data and see if we can't find something that will be of interest to you."

So he agreed. I sent a message to Isabel Kelly in Mexico, Richard Adams in Guatemala, Charles Erasmus in Colombia, Ozzie Simmons in Lima, and Kalervo Oberg in Sao Paolo. I explained the problem. I said, "Get in touch with the chief of mission--they're expecting you--and find out the nature of their health programs, and what problems they're encountering. And see what you can figure out. Send me the data, and I'll put it together."

So they gathered data for two or three months and sent their notes to me. From these notes I edited a mimeographed paper of about a hundred pages, *A Cross-Cultural Anthropological Analysis of a Technical Aid Program*. We found all kinds of interesting things. For example, the United States health program was trying to introduce public health centers, the same type we had in this country, which was a fairly new concept at that time, emphasizing preventive medicine over curative treatment.

Evaluating the ISA Latin American Program

Foster: This paper was, you might say, a bombshell. As soon as it came out I had a call from a man named Henry van Zile Hyde, who became my public health godfather. He was a physician who had gotten into international work early in the game. He was the head of the Health Division of the IIAA--he had been on leave when I first approached it. He was very excited about what we had done. He said, "We're about to establish a ten-year evaluation of our whole program in Latin America. The leader of the evaluation team will be Wilton Halverson, head of the Department of Public Health in California, and it will include other top figures in public health, such as George [K.] Strode of the Rockefeller Foundation."

He continued, "We'd like to have your people join us, and we'll pay their salaries if you will." So for the next year we were all supported by the Institute. I started out with the team, I spent three weeks in Salvador, and that's when I first began to learn about the culture of health and realized there is a bureaucratic culture of health, just as much a culture as any culture of a primitive village.

Isabel Kelly came down from Mexico and joined me there. We worked for the team. They were all top-notch people, and we enjoyed them greatly. Then I had to go back to Washington. The team went on to Colombia, where Erasmus joined them. Then to Peru, where Simmons joined them, and then to Brazil, where Kalervo Oberg joined them. Our people worked for three or four months gathering data.

I went down to Peru in the spring, 1952. With Ozzie Simmons, together we moved on down to Chile, where we spent three weeks or so. I learned a great deal there because that was the first place that I had ever gone around in a strange country, knocking on doors, saying, "Do you know about the health center?" And, "If so, do you like it?" And asking questions and going to the health center and seeing how the public health nurses dealt

with the patients, how the doctors dealt with the patients. A wonderful opportunity to do field work.

We didn't experience any of the tension that often accompanies anthropologists in applied work because we were learning, and we believed the program was good. We approved of what they were doing, trying to improve health standards, so there was no ideological conflict. All the people we dealt with accepted us at face value and believed we were part of the team, as we were.

Then, in June of that year, all the chiefs of mission of the eighteen parties of all the countries that had this program came to Washington for a week. And on one day, I presented our findings. That was one of the great days of my life and a great day, I think, for public health, too. You've never seen such enthusiasm. We were able to explain a lot of things that the public health personnel had been knocking their heads about.

For example, we would sit in the room, watching the doctor and the nurse talk with a patient. We'd sit while groups of expectant mothers were listening to lectures about pre-natal care. I remember once in Temuco, Chile, when the public health nurse was translating directly from an American textbook, and she said "every so many hours" you must do something or other. I asked if I could ask a question. I said, "How many of you have clocks or watches in your home?" None of the hands went up. I suggested that the instructions could be given in terms of when the church bells ring. That's an example.

The major thing we concluded, and they accepted it, was that failure to treat sick children was the biggest reason people would not go to the health center. The health center would--if the kids had been to what is called a "well-baby clinic," then they were registered, and if they fell ill the parents could bring them in and they'd be treated. But if a mother walked in off the street with a sick child, the child would not be treated. That was to make the parents register the children.

That angered the people, because in their minds a health center, or a hospital, is a place where you go when you're sick, not when you're well. One of the main things we urged was that they reconsider, and that they break away from the traditional American dichotomy of preventive public care and private curative care. They've done it to a considerable extent. I think that was one of our major contributions.

We found that, whereas the health people and we had assumed that the primary barriers were what I call cultural barriers,

related to the prevailing beliefs and practices--and those were important--that it's just as important to recognize that the services offered are not as good as they could be. For example, loss of time--if you're a busy mother in a poor community you can't spend all day sitting in a health clinic waiting for the doctor to come who's probably off chauffeuring around some visiting Americans, like us. That was one of the major complaints: the lack of consistency in the scheduling of service.

Riess: You were observing how the service was being offered. Had you made assumptions about the problems?

Foster: We didn't know what the problems were. We assumed that we would find that the major problem--I think we tended to assume that it must be a very good program: lots of money behind it, skilled personnel, and the type of program that was working well in this country, or fairly well. So it was a surprise to us to realize that the barriers to change in the innovating bureaucracy are just as frequent and inhibiting as in the target group, the community that is felt to be in need of help. This was the germ of an idea that I did not fully develop for thirty-five or more years, in a paper titled "Bureaucratic Aspects of International Health Agencies." In this paper I suggest that there have been three stages of awareness of the problems inherent in technical aid programs.

The first stage, what I have called the "Silver Platter" model, was based on the assumption that the "best" programs and policies devised in the technologically-advanced countries will work well in every type of community. It was further assumed that the recipients of help in developing countries would immediately recognize the advantages of the new ways, once exposed to them, and that given the opportunity they would quickly adopt them. This, of course, proved to be erroneous.

By the mid-1950s this early ethnocentric view of how to carry out our technical aid began, largely as a result of anthropological input, to give way to an approach that postulated that the major problems inhibiting change in target groups are rooted in their social and cultural forms. Hence, the planning of effective programs required sociocultural research to identify the "barriers" to change found in the receiving societies, and the reformulation of programs that fit into the local life patterns. I call this the "sociocultural model," and it is still the dominant model among planners.

As far as this model goes it is correct, but it is incomplete. Little by little we have come to appreciate that failure to understand the social and cultural forms of the

innovating bureaucracies is also often the cause of failure. The third stage, the "bureaucratic model," postulates that the most effective technical aid programs assume that it is just as important to understand the culture of the innovating bureaucracy as the culture of the recipient group.

The first two states are easily accepted by planners and technicians, because the locus of the problem is "out there." The third stage is resisted by many people and organizations because a major part of the problem is so close to home. It reminds one of Pogo's "We have met the enemy and he is us." In my WHO (World Health Organization) consulting in the 1970s and early '80s I found strong resistance to the idea that the organization's structure and standard operating procedures were major factors in the outcome of many programs. [e.g., "World Health Organization Behavioral Science Research: Problems and Prospects," 1987] I'm told the situation is still pretty much the same.

Riess: In your 1951 report, the *Cross-Cultural Anthropological Analysis of a Technical Aid Program*, you told them that the news was not good?

Foster: No, we wrote our reports emphasizing the positive. That's one thing we learned, you can always emphasize the positive. You can say, "In general, services are very good. There are a few exceptions." The canny people catch on.

We were generally very enthusiastic and supportive of them in their work, and they sensed it. In general, that's the way I have approached applied work, as against "let's get the goods on the bastards"--that's the way I sometimes summarize the other approach. You're so strongly supportive of the underdogs that you assume everything is a plot against them: they're being victimized by terrible people.

Riess: It almost goes to good manners. First say something nice.

Foster: Well, it's not so much a question of good manners as it is a reflection of how you feel about the people. I can disagree with people, and feel they're doing it wrong, and still empathize with them and feel they're doing the best they can, according to their lights, just as I am doing the best that I can, according to my lights.

Fifth WHO General Assembly

Riess: And after that report?

Foster: That led to my participation in the Fifth World Health Organization General Assembly in May of 1952. Henry van Zile Hyde, who was very enthusiastic and supportive of my work, had me named an advisor to the American delegation to the Fifth World Health Assembly. The WHO Building had not yet been built in Geneva. We met in the old League of Nations Building. The head of the delegation was Ernest Larson, the head of the AMA [American Medical Association]. He was a North Dakota physician. The delegation also included C.E.A. Winslow, a famous public health pioneer from Yale University, as well as the novelist Fannie Hurst. There were also other "advisors," like me, technically not members of the official delegation.

In the course of this experience I learned a lot more about the functioning of bureaucracies. I was very angry about the American position. The American policy was officially and formally opposed to any action on the question of family planning and birth control methods, because we didn't want to irritate our Catholic friends in Latin America. I thought it was a terrible mistake, and that we ought to face up to the problem.

Riess: And it hasn't changed.

Foster: It has changed a good deal, but it hasn't changed as much as it will change ultimately.

These countries were asking for help. They formally pleaded with WHO to give them help, and we not only refused to do it ourselves, but we nixed discussion of the topic within WHO because of our power in shaping the organization. A great shame.

Riess: Did you speak out about that?

Foster: I didn't speak out about that, no. It was not my job to speak out about that.

One afternoon, I spent an hour or so summarizing our findings in the Latin American research. That was my main contribution, introducing the idea that it helps if you know about the culture of the people you're trying to help, and even better if you know a little about your own culture.

Riess: Did they talk about population as a problem?

Foster: Some of them were concerned, yes. It was a thinly-populated world then, compared to now, but already it was apparent that the population problem was going to get away from us. It certainly has.

Riess: Your trip to Geneva, was that another flying trip?

Foster: No, I went on the "Nieuw Amsterdam"--that was a wonderful ship. I was supposed to come back on the same boat, but I had to get back for a meeting, so I flew back on an Air France Constellation. That's when I first had high cuisine in the high skies. That was a wonderful flight.

Rethinking Applied Anthropology

Riess: You introduced the idea of applied anthropology in international public health programs, that it was sort of anathema, but it made sense at the time. What has been the history since then?

Foster: A few anthropologists were beginning to show interest in the question of how their knowledge of facts and their research techniques could be evaluated in developing health programs. WHO had had Cora Du Bois on its staff for a year, about 1949. She was the first anthropologist ever hired by WHO. She was completely disgusted with it. They never asked her to do anything. She never followed up.

The first real breakthrough, I would say, was Benjamin Paul's casebook, *Health, Culture and Community*, which was published by Russell Sage [Foundation] in 1955. The Russell Sage Foundation had hired an anthropologist, Esther Lucille Brown, at least ten or fifteen years older than I am, a Yale Ph.D., about 1933, for a six-month assignment. Esther Lucille is the least-known name among medical anthropologists, and she ought to be one of the best-known, because she was the person who really sparked anthropological interest in health problems.

That six-month job at the Russell Sage Foundation happily turned into a lifetime appointment. Initially she worked on sociological problems concerning the nature of bureaucracies. But after the war she realized that social science had a great deal to offer health, and she persuaded the Russell Sage Foundation to give money to a remarkable person named Hugh Leavell, professor of public health policy at the Harvard School of Public Health, with the understanding that he would add a behavioral scientist to his department. Benjamin Paul was then a junior member of the faculty

at Harvard, and he was hired by the School of Public Health for several years. His main opus was *Health, Culture and Community*, a book that's still very well known and used.

All during the late 1950s and 1960s there was a series of conferences and meetings in which we were sort of feeling our way, so to speak, trying to bring structure into what we recognized was going to become an extremely important anthropological specialty. We wondered what special training young anthropologists going into this as yet unnamed field would need. Margaret Clark, for example, was my first graduate student to complete a Ph.D. (in 1957)--I think she was the first anthropology Ph.D. in the world to write a dissertation on a medical anthropology topic. [*Health in the Mexican-American Culture: A Community Study*, UC Press, 1959] The term medical anthropology had not yet been invented, it was not widely used until about 1962 or '63.

Riess: What was it called before then?

Foster: Applied anthropology in medicine.

Community Development for the ICA

Riess: In 1955 you were sent by the Community Development Division of the U.S. International Cooperation Administration, ICA, to India, Pakistan and the Philippines.

Foster: That is interesting from the standpoint of UC Berkeley history because Paul Taylor was a member of that team.

Riess: But first we should conclude our discussion of your work in Washington and get you out to California, to Berkeley!

Foster: I came to Berkeley in January of '53, right after this year of activity that included Geneva and the ten-year evaluation of the United States Public Health Service Institute of Inter-American Affairs evaluation team. Then, for several years, I went to Washington quite often, as a member of the Health Committee of the U.S. Foreign Operations Administration, which later became the International Cooperation Administration, and in 1961, under [John F.] Kennedy, became AID, the Agency for International Development, where, mercifully, it has had the same name ever since. Until then, it was also called Point Four. That was the common shorthand to identify the early American technical aid program.

Well, by chance, I wrote an article called "Guidelines to Community Development Programs." It was mostly the public health stuff. It was published in *Public Health Reports*. It appeared early in 1955, and it was picked up by a man named Louis Miniclier--who quickly became a very good friend until he died in Spain fifteen years later. He was in charge of a major unit of what became the Agency for International Development--the Community Development Division.

Community development was a movement designed to stimulate poor communities to improve their social and economic conditions through self-help and community-wide cooperation using local resources. During the 1950s and 1960s it was viewed as a way to begin to solve problems of poor villagers in developing countries "on the cheap," so to speak. The movement was extremely popular among planners and budget officers during those years.

And the fact that by pure chance I'd put my health ideas in a wider context and called it community development, rather than health, caught his eye, and he asked me if I would be interested in joining a group that was going to visit these countries on a round-the-world trip. I said I would. He said, "Do you know anybody else who might be suitable?" I said, "I've got just the man for you. His name is Paul Taylor." So that launched Paul on what was his major interest during the last years of his life, I'd say.² He became a major figure in Louis' program. (See further discussion, pp. 244, 254.)

²Paul Schuster Taylor, *California Water and Agricultural Labor* [Volume II], Regional Oral History Office, The Bancroft Library, University of California, Berkeley, 1975.

VII UNIVERSITY OF CALIFORNIA, BERKELEY, 1953

Thoughts on Returning to Academia

Foster: I came to Berkeley, oddly enough--. Well, still in Washington, let me say that the Institute of Inter-American Affairs supported the Institute of Social Anthropology for a whole year, giving the money to the Smithsonian. After that, we were told, "We'd like you all to continue with us, but we'll have to put you on direct hire on our staff."

I realized that I was getting near the point where if I was going to be an academic, I would have to get back into academic life. Isabel Kelly and Kalervo Oberg and Erasmus and Simmons stayed with the Institute for varying periods of a year up to seven or eight years, but I decided that we'd better fold the Institute, which we did. I had an invitation through Gifford, Edward Gifford, who was the director of the museum [Museum of Anthropology, UC Berkeley], who had been a good friend for many years. He had persuaded Ted McCown, who was chair of the department at that time, to hire me on a part-time basis for the spring semester of '53.

So Mickie and I and our two kids drove across the country in January. We were icebound in Kearney, Nebraska, for two nights, glaze ice. That was interesting. We were about three or four miles east of Kearney, Nebraska, when a freezing sleet started to form on the windshield. We were just able to creep in to town and get to a motel, where we put up for the night. The next day, everything was just sheet ice, no traffic at all. So we decided we'd stay another day, which we did.

Several days later we crossed Donner Summit on the old road, in a blizzard, and stopped and listened to [President Dwight D.] Eisenhower deliver his first inaugural address. And when we came down into the Sacramento valley everything was green, lush. It was like coming into paradise. We rented Dave [David G.]

Mandelbaum's house that spring. Dave was on leave at Cambridge. So it was nice to have an anthropologist's house.

Riess: Had you been keeping in very close touch with the department over the years? And in what ways?

Foster: Yes, I'd been in touch with them because Mickie's parents moved to Berkeley from Washington in 1943, so after we came back from Mexico, we'd be out here every summer for two or three weeks of vacation. Mickie's mother would always throw a nice party for the department staff. So I was in touch.

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Foster: Dave Mandelbaum had been Herskovitz's first undergraduate student, so I knew him. And I knew Ted from the time I was a graduate student, so I didn't feel strange at all. And Giff [Edward W. Gifford] I had known--we had had a wonderful weekend with him and his wife, Delila, in 1946, along the west coast of Mexico, in Colima. Giff's retirement in June 1955 was what made possible my appointment--in fact I probably owe my appointment here to him, because in the spring of '53, when I came on a part-time basis, he's the one that suggested to the department that they ask me if I would be interested in coming out for a semester, and I jumped at the chance.

Riess: Had you considered other universities for your return to academia? Had you been courted by other universities?

Foster: The School of Public Health of the University of Pittsburgh, Thomas Parran, who had been the Surgeon General, invited me out to give a talk in that tower of learning. And I realized I was being looked at, but I didn't encourage it to go further.

At some time, when I was in Washington, I was looked at by the University of Michigan. I think that was probably Jimmy Griffin [James Bennett Griffin], an archaeologist whom I had known very well in Mexico. We'd always been good friends. Nothing was said about a job, but I'm sure they were looking at me. But I was not particularly interested in that, either. I wanted to get back to California in the worst possible way. I just had fallen so in love with Berkeley when I first came out. It was the only fit part of the world to live in, I thought.

Riess: How was Berkeley viewed academically in 1953?

Foster: It was a top school then.

Riess: It was the top school in anthropology?

Foster: It was not the top school. I'd say Chicago was the top school. But that was a great period to be here--we were developing. By the early 1960s, we were beginning to be ranked number one, a source of considerable pleasure and pride to think that I was a member of that group.

Split Between Public Health and the Museum of Anthropology

Foster: But to go on back to my own case, I got in touch with the public health people right away, Bill Griffiths and Dorothy Nyswander, particularly, who became good friends. They had heard about my work in Washington from public health colleagues, so it was a natural tie. I began giving lectures to some of their people, and they expressed interest in having me--Chuck [Charles Edward] Smith was dean at that time, of the School of Public Health. He was a great person.

The department found money to carry me the next year. And during that next year I was approved for a joint appointment, one-third in public health and two-thirds in anthropology, which would have been ideal for me. So I was in clover, you might say, thinking--I couldn't believe I'd not get the job. Well, nothing happened and nothing happened and nothing happened. The end of the year came, and I still wasn't appointed. At that point, anthropology took me on for a second year.

I later asked Chuck Smith--I said, "Chuck, what happened? What happened to that proposal?" He was embarrassed, and he replied, "Well, you know, Ted McCown was chair, and he didn't do anything, and we decided that he just wasn't interested in the program, and we were one-third as against two-thirds, so we were embarrassed to raise it." Ted had just sat on the documents, never signed them or anything. He wasn't antagonistic to me, he was paralyzed. He was associate dean with Lincoln Constance at the time. Lincoln thought he was a great associate dean, and I'm sure he was, but he was incapable of making final decisions. Lincoln subsequently came to realize that. I've never held it against him, but it was irritating at the time--because I knew it wasn't personal.

By the third year, by 1954, I had been there three semesters, still drifting. Then Giff retired in 1955, so the department had a slot, and they put me in as director of the Museum of Anthropology. I insisted I be "acting director" because I didn't want to be stuck permanently with the Museum. But to all intents and purposes, I was the director of the Museum. So I've

always said I was the anthropologist who came to dinner, having five semesters before my appointment became final. [laughs]

And I'm glad it turned out to be all anthropology, because if I had been in the public health, I would have been split too much. I think joint appointments are in some ways very interesting, but they're also terribly consuming because both departments assume they'll have the full time of the individual for departmental affairs. It would have been too much, to have to go to two staff meetings a week, and two social functions, everything duplicated.

From 1954 until about 1963 or '64, I gave a seminar every year in public health. I held the title "Lecturer in Public Health," a courtesy title that I was given, and I was listed in the catalog under Public Health, as well as Anthropology. That was terribly interesting. Dorothy Nyswander and Beryl Roberts, who died tragically very young, who had come from Harvard to replace her, and Bill Griffiths--they were all superb people to work with. Practically everyone on the faculty of public health--I liked them all.

Margaret Clark became my graduate student, she was the first one [anthropology]. We had a grant from the Rosenberg Foundation for her to do a study of a Mexican-American community in San Jose [California], Sal si Puedes. That was a very successful study and book. It gave her a tremendous start in life. She ultimately, of course, was responsible for the medical anthropology program at the University of California, San Francisco, and became president of the American Anthropological Association. A very first-class person.

Riess: Did you continue your seminar?

Foster: Until about '63 or '64, by which time they had brought Andie [L.] Knutson in--they had their own behavioral scientist. And I was more and more involved in matters in anthropology.

Riess: "Behavioral scientist." Another term for anthropologists is behavioral scientists?

Foster: And sociologists. The Center for Advanced Study in Behavioral Sciences in Palo Alto is linguistics, anthropology, sociology, economics, philosophy, you name it.

Planning the Future Kroeber Hall

Riess: To back up, what did you do at the museum? Was it like running the Phoebe Hearst Museum of Anthropology now?

Foster: In a way, it was much simpler. The museum was open only for a couple of weeks a year: two weeks in the spring and two weeks in the fall. We had two small rooms, where we put up exhibits to illustrate the course work of the teachers. During the rest of the year it was closed to visitors. The museum was in the old civil engineering building, and it's a wonder it didn't collapse under the weight of all of that stuff.

Riess: Was there a feeling on your part that it should be open more?

Foster: Well, I wasn't worried about that because we were already drawing up the plans for Kroeber Hall, where we would have a museum with a large exhibition hall, and that is what interested me particularly. I was the departmental representative working with the campus architect, Louis DeMonte, and the architect, Gardner Dailey. It was interesting. Gardner Dailey was very broad-gauge.

But I've got to speak about the role of David Mandelbaum first because that was very important. David knew that the museum needed a lot of thought and careful planning, and he recognized that none of us had any idea as to what a modern museum was. So he persuaded Eric Douglas, who had built the Anthropology Department of the Denver Museum of Natural History into one of the finest in the country, to come to Berkeley and spend several weeks teaching us what we needed to know for the new building. Eric Douglas, who was then dying--he had already lost his left arm to cancer, a very sick man--came out and spent about a month with us. We learned an awful lot in that month about what a museum hall should be and what things like that were.

It turned out that we had done everything wrong in the plans. (Preliminary plans had been drawn up when I took over.) The storage, for example, was a stack that went right up through the prime space of the building, right to the roof. Well, we got rid of that fast, we got it all down to the basement. But it was because of David's perception that we were able to get Eric Douglas, and I've always been sorry that something is not named after David in the Museum. It should be. He played a very formative role. He was chair at the time. He took over after Ted was eased out.

Riess: Why was there money at this time to build a building? Is there a story?

Foster: I'm told that Kroeber had the opportunity to take anthropology into the Life Sciences Building when it was opened in 1933, but he said, "No, I'm going to hold out for a building that will be anthropology." The turn of the Anthropology and Art departments to have new space came up at the same time, in 1953 or 1954. So and Art and Anthropology building was in the preliminary stages when I officially joined the department in '55. The museum then was administratively a part of the department--the museum budget was a part of the department budget. That's not the case any longer. So my role as contact man with the architects was not only for the museum but it was for all the offices, for the library and everything else in the department.

David again played a major role there. He said, "We've got to have a student-faculty lounge." So that's what's the Gifford Room today. But the architect said, "You can't have it because it's not in the original specifications." I said, "Well, how about taking six hundred feet out of the space allotted to storage, and putting it in here, and we'll call that the student-faculty lounge?" They [the architects] said, "You can't get away with that." Then David suggested, "How about making it the Laboratory for Interpersonal Relations?" [laughter] Which is the way it appeared on the final plans. And we got away with it. It has been a lifesaver. When I think that room would have been storage space for pots and pans--I think how lucky we were.

Riess: You say it was good working with Gardner Dailey?

Foster: Yes. His idea of a museum--Eric Douglas had taught us for a museum of anthropology we need plywood walls. We need things you can drive nails into. We need moveable partitions. We don't need wainscoting and fancy canvas walls. Gardner Dailey's idea of a museum was an art museum, with skylights and everything, and nicely finished. We said, "Mr. Dailey, that's not what we want."

He shook his head and laughed, and he said, "Well, all right, if that's what you want, you'll get it." He was very adaptable. He was much more adaptable than Louis DeMonte was. But--I see Louis had to protect the university, whereas Gardner Dailey could let his imagination run. I must say we got an awfully good building. Structurally it's very sound, too.

Riess: And the demarcation between art and anthropology? Is there any flow between the two?

Foster: They were marked off from the beginning. The artists were very angry at the anthropologists because we got our Laboratory for Interpersonal Relations, and they hadn't gotten one. And they

complained. They finally got an inside room that they use for such purposes.

But the Library--originally, it was the Art and Anthropology Library. Somehow, we eased them out and so it became only the Anthropology Library, which is very lucky.

Riess: And you would think art would claim museum space, but all they have is the little gallery, the Worth-Ryder Gallery, I guess.

Foster: I suppose the art gallery, the University Art Museum, across Bancroft Way, was coming on line. I don't remember that was a point of contention.

I remember the artists were awfully difficult to work with, as artists are supposed to be. They were always bitching about one thing or another. After we moved into the building and had been in it for six months, they sent a letter to Gardner Dailey to gripe about a lot of things. I was just horrified. I was chairman then, and so I wrote him a letter, saying, "I don't know about others, but for the Anthropology Department, I want to tell you, you've given us a marvelous building. Our efficiency has gone up. We love everything about it."

To my astonishment, about two weeks later, he came to my office with a secretary, a nurse--he was a very sick man--and he said, "I wanted to thank you for that letter." [with emotion] It made me realize how rarely we say things we ought to say more often.

Riess: I think that's right.

It has always seemed like the anthropology building because of the totems in the courtyard.

Foster: Yes, it's much more anthropology than--the name Kroeber, and the "Lowie" Museum, as it was for the first years--it's too bad it's not still the Lowie Museum.¹

¹John Rowe was chairman of the department about 1965 or so. The art students had some old pieces of wood, stuck together, and stuck down in the lobby, and John sent a news story to the *Daily Californian* saying that "the remains of the wreck of the Hesperus has been found." It looked like the wreck of the Hesperus, to the non-trained eye, at least. The art department was not amused. But we have gotten along pretty well with them.

Riess: So this was an important role for you. You had an impact on the design of this place that you've been in ever since.

When you returned, Lowie and Kroeber were still here?

Foster: Kroeber lived to see the building. He was in it about a year. He had an office on the second floor on the west side. He needed to rest a lot. How times have changed: the university let him have a bench about eighteen inches wide, with a thin pad, that he could lie on--couches then were prohibited in faculty offices, presumably to discourage illicit goings-on between male professors and female students.

[looking at 1956-1957 catalogue] It was an amazingly small department. Heizer and I had been fellow graduate students. McCown was away much of the time I was a graduate student, he didn't take his degree until about 1940, I think. Mandelbaum, of course, came after the war. John [H.] Rowe came in '48, I believe. As I say, I had known him first in the Institute of Social Anthropology. Giff and Delila and I were good friends from the time I was a graduate student. And Kroeber and Lowie. [Ronald L.] Olson.

Riess: It was a very California department, wasn't it?

Foster: Very much so, yes.

Riess: I thought there was a tradition of trying to bring in outsiders--I mean, not just in anthropology, but at Berkeley.

Foster: Well, there is. But you can get too many outsiders. I think the department had an un verbalized tradition that up to a third, or at least a quarter of the professors, should be Berkeley products. I accept this tradition myself, because I think it is important from the standpoint of stability to have staff members who know and subscribe to the history and tradition of the department. About three or four years after this, when we had others from outside who tried to change the whole department and ignored the history, we had a terrible time. That's a bit of scuttlebutt I'll spill later.

The Inroads of British Social Anthropology

[Interview 6: January 13, 1999] ##

Riess: You said before we began that you prepared for this interview by reviewing your line-a-day books. What does that mean?

Foster: Six days before I was married, on the first of January of 1938, I started keeping a little line-a-day, just telling where I was and the principal things that happened. It has been very useful in reconstructing my life; since I never kept a journal it's the closest thing I have to a journal. It enables me to check with my biographical records and pin down important dates, such as the day we moved into Kroeber Hall and things like that.

Riess: What an excellent record!

You have said that in 1935 Berkeley's training brought out the best in a diverse group of scholars, that the department was a supportive environment, that students were not as competitive as they are now.² What happened to anthropology in the ten-plus-year period from when you graduated to when you returned to Berkeley?

Foster: That was the period in which British social anthropology became the standard for America, those ten years, or beginning a little earlier. The big break in anthropology, I'd say, came about the time I graduated from Northwestern in '35, when the University of Chicago, under Fred [Frederick Russell] Eggan and Robert Redfield, brought Radcliffe Brown to Chicago, where he taught for several years.

And structure and function, which is the basis of British social anthropology, became all the rage among many anthropologists in this country. It was a very important step forward, but it went too far, I think. It rejected history, which I think is basic in any understanding of how people behave and why. But I'd say that was the big development during the few years after I graduated, and before I came back to Berkeley. That led to the big blowup when I was the chair--that's another story, though, we'll come to a little later.

²Video interview with Charles Wagley, 1983, in the "History of Anthropology" series produced by the University of Florida's Department of Anthropology in conjunction with the Wenner-Gren Foundation for Anthropological Research.

- Riess: Were you keeping up with this, and aware of the implications of this in that ten-year period?
- Foster: No, I wasn't, because the first couple of years after taking my degree I was trying to keep up with my introductory lectures, and then I was in Mexico, where I was not exposed to these ideas. And then at the Smithsonian, social anthropology was not at the top of the list there, either. So I was not really exposed to it. I didn't realize how much it had influenced mainstream anthropology in this country. I was still very much a Kroeberian and Lowiean historical anthropologist.
- Riess: What names are associated with structural anthropology?
- Foster: Radcliffe Brown would be number one, and the London School of Economics. Radcliffe Brown, particularly. They stress kinship and structure of societies, the way societies are put together. It was based particularly on African material, which is not surprising, since that was where the bulk of the Empire was. Raymond Firth to a lesser extent, though I never felt that he was really a structure and functional anthropologist.
- Riess: They're odd words, in a way, so abstract, "structure" and "function."
- Foster: Well, they're pretty basic. Structure refers to the way the system is organized in any society, and in non-Western societies, tribal societies, it's particularly kinship. Kinship systems have always been a basic part of anthropology, from the very beginning. Function has to do with how the parts fit together and the whole thing works, like the function of an automobile engine.
- Riess: When you first heard this, wouldn't you argue that that's what you were always doing?
- Foster: I think that's what everybody does, in all science, when faced with something drastically new. Certainly, that's true in anthropology. I've seen Margaret Mead arguing that the present generation is just reinventing the wheel. I think that's true of everyone, they try to justify what they've done in terms of this new term for it.

But British social anthropology was quite different from American anthropology. It was non-comparative, which was its weakest point. American anthropology has always been very much a comparative discipline. That is, we deal with forms of behavior, traits, and we ask ourselves such things as, what is the distribution of this trait? What variations are found in different societies, i.e., what are the core characteristics

always (or almost always) found, etc., etc.? We draw our conclusions, not from single societies but rather from the patterns apparent in a cluster of societies. I think my work in ethno-history of humoral medicine in the New World illustrates that beautifully.

There's a British social anthropologist, Audrey Butt Colson. She argues that the concept of foods and medicines having metaphorical qualities of temperature--that they are "hot" or "cold"--the unifying element in almost all Latin American popular medicine, must be Indian and was adopted by the immigrants to the New World from Spain, because she found a so-called primitive tribe in Venezuela that has it. She says the Indians could not possibly have been exposed to the humoral ideas brought to the New World by Spaniards, although her own account reveals how these ideas might have reached them. She ignores all of the comparative data and jumps to this cosmic conclusion. If she'd just read a little bit about humoral medicine in various parts of the world, she'd see that she couldn't possibly be right.

Riess: I thought you had already established the basis for it.

Foster: I thought I had, too. But it's funny, when people make up their minds, the evidence just annoys them, they don't pay any attention to it. There are people who still argue that hot and cold--in folk medicine, the metaphoric hot and cold qualities--must be Aztec. Unbelievable. I just can't see how they can possibly have such ideas.

Riess: That's a topic, the argumentative nature of anthropology. Your writings about peasant societies have been controversial. I don't know whether this is a good time to talk about that or not.

[tape interruption]

Validation, An Aside on Interpreting Evidence

Foster: I think when you say my position has been contentious, you're referring particularly to the "Limited Good" article?³ And I must confess, I don't understand that either. People were either wildly enthusiastic or they just thought it was the pits. Nobody seemed to be halfway, saying "it may be true, or it may not be."

³"Peasant Society and the Image of Limited Good," *American Anthropologist* 67: 293-315.

I just can't understand the reaction. About 50 percent of the people who commented thought it was the best article they had ever read, and about 50 percent thought it was the worst. Nobody said this was one of the twenty best or the twenty worst.

Riess: Laura Nader in the Kroeber Anthropological Society papers, says, "His ideas on these topics [on peasant world view] have been hotly debated and widely cited, and remain among the most controversial in the anthropological study of peasant society. Although no commentators are neutral on the subject, it is curious that some accuse Foster of economic determinism while others charge him with cognitive determinism."⁴ Where does all this come from?

Foster: "Limited good" has become a standard term, the way Redfield's "folk-urban continuum" became standard. I'd say to a considerable extent it is more widely accepted now than it was at the time it came out. I don't feel that--surely, it has not been ignored, and I think it generally is at least partially accepted as a valid position.

Riess: When you talk about accepting something as a valid position in anthropology, is it that the profession has stepping stones and building blocks, and so if you accept it, you have to accept it across the board?

Foster: No. I think the point I should make is that anthropology is not like a hard or exact science. You can't have a laboratory test and expect people to come up with the same results. All you can do is interpret your evidence. The most economical interpretation, I think, is the most valid one. But about all you can say is that this is my best effort to date to explain this phenomenon. Certain points of view are generally accepted, in anthropology in general, and others are not. But there's no way you can prove a hypothesis.

Riess: You are saying that doctoral students wouldn't set out to prove or disprove a hypothesis.

Foster: No. That's not really the way anthropologists work.

I think there are anthropologists who have encountered some experience in the course of their own field work which either supports it, or conforms or does not conform to it, and then they write on the basis of what they found, their position. But I

⁴"Will the Real George Foster Please Stand Up? A Brief Intellectual History," by Eugene Hammel and Laura Nader, in *Essays in Honor of George M. Foster*, Kroeber Anthropological Society Papers, Berkeley 1979.

don't think anybody has tested it as a doctoral dissertation, I don't think you could. I think it would be a waste of time for a young anthropologist.

Expanded Field Research Program at Berkeley

Riess: When you graduated in 1941 there was a pattern at Berkeley of summer research, rather than the year in the field. Was that the pattern when you came back?

Foster: No, by the time I came back, in those eleven years, the standard pattern of field research had become a year or so, which is the British system. That is, if one were doing a community study, which no one had done when I was a graduate student here in Berkeley, one would expect to spend at least a year in the field, gathering data. The time I spent in the field, which was three months, was a ridiculously limited amount of time. But, unlike a lot of later students, I didn't consider my dissertation to be my magnum opus. It was simply the last test among the many I had taken along the way to taking the Ph.D. degree. One didn't get a job on the basis of a dissertation in book form, which is almost standard now, you might say.

Riess: It became a very different profession, in that eleven-year period.

Foster: Oh, yes. It changed a great deal.

Riess: When you were brought back, was there a sense that change was needed? And you might be the person to make it?

Foster: No. They just felt that I was a fairly competent anthropologist. I was a visitor for two and a half years, as I said. I thought I was going to be in the School of Public Health one-third time. In retrospect, fortunately that didn't work out. So I was full-time when I replaced Gifford as director of the Museum of Anthropology. But I was not interested in being the long-time director, so I insisted on being appointed acting director of the museum, which I was for the three years, until I became chairman of the department.

More on Kroeber Hall and the Museum of Anthropology

Foster: I must say, I worked my tail off in the museum, particularly in planning the new museum. Not long after I took over, when they were first planning the museum, Mickie and I made a trip to Santa Fe, where we looked at the folk art museum; to Denver, where we looked at the Denver Museum of Natural History; to Milwaukee, for the Milwaukee Public Museum, which was perhaps the best anthropology museum in the country at that time; to the Field Museum in Chicago; to the Peabody Museum at Harvard; to the American Museum of Natural History in New York; and to the University of Pennsylvania Museum in Philadelphia--trying to figure out what it meant to run a museum, what a museum should try to do. We were educating ourselves in museology, you might say.

[laughs] I suppose the most important finding we came back with was something called Flexowax, which was a wax--you could stick your specimen against a wall with the wax and it would stay there.

Riess: Are you kidding that that was the most important thing?

Foster: Sometimes I think it was. No, we learned a great deal about how things should be exhibited. I have always found it interesting to learn new aspects about anthropology, and this was something that I enjoyed while I was doing it, but I'm glad I didn't have to do it any longer than three years.

Riess: Was the museum designed mainly to support the academic mission, or did you have hopes for more?

Foster: Well, until we built Kroeber Hall its role was to display specimens that would be used in talking about material culture in the basic anthropology courses: the casts of the skulls of Neanderthals, the Java Ape man and the Chinese *Sinanthropus*, human skeletons, artifacts--pottery and things like that--that might be talked about in class. It was not a public museum at all.

It had been in San Francisco until 1931, I believe. Then it was moved to the old civil engineering building here at Berkeley where it stayed until we went into Kroeber Hall in '59. But yes, Kroeber Hall was designed to be a public museum. It had a 6,000-foot public exhibition hall, and adequate space for the catalog and for preparators to access specimens and to work on exhibits. And there's the bookstore, of course.

Once it was decided to build Kroeber Hall--formally named Kroeber Hall after it was finished--the plan was to have the museum in the building, as a part of the building.

Riess: And has it waxed and waned in terms of the public outreach effort?

Foster: Well, it's not a big draw, but it gets a good many people. It's listed every Sunday in the *Chronicle*, along with the California Academy of Sciences and SFMoMA. A good many people come in, and it has good exhibits. It doesn't have the money to have as many or as elaborate exhibits as it would be nice to have, but it does very well, I think.

Riess: What was its endowment?

Foster: It's never really had any endowment at all. It's very much a hand-to-mouth operation. When I was acting director of the museum, and when I was chairman of the department, the museum budget was part of the departmental budget. I think it had a hundred thousand dollars, which is not very much to run a big museum. But at some time since then it has split off, and now it has its own independent budget.

The utility of the museum depends on the interest and ability of its director. I'd say Burton Benedict was by all means the best director we've had, in my experience, because he gave most of his time to it. Most of the other directors have given it a little bit of attention, but have assumed they could leave most of the responsibility to other employees of the museum.

Riess: So it has always been a part-time academic appointment.

Foster: I forget whether the FTE was full-time or not. It was understood that the director would be a regular staff member and would teach and direct students in research, along with the activities of running the museum, but would not teach as much.

"The Nature of Culture" Course, Humoral Medicine Insights

Riess: So you worked on the museum, and you were teaching in public health, and in anthropology?

Foster: I was teaching, but it was a visiting appointment. It had no commitment for the following year.

Riess: And your tenured appointment came through in 1955.

Foster: Yes. I was on that trip around the world with Paul Taylor, on community development, when I got a cable saying I had been appointed. That was an awfully nice feeling, after having been on tenterhooks for a year and a half or so.

Riess: You created new courses at Berkeley? Your year-long seminar on acculturation?

Foster: I think I began teaching acculturation while I was a visitor. I imagine I gave it almost from the beginning. I'll have to check and see.

Riess: You taught "The Nature of Culture: Anthropology 118" and then a course in "Europe and the Mediterranean."

Foster: That was one I started. But "The Nature of Culture" was one that I took over from Kroeber.

Riess: And how do you think you changed it?

Foster: I began teaching it at UCLA, the year I was there. I pretty much compressed my two semesters of course notes of Kroeber into one semester, and I didn't do nearly as good a job as he did. He had incredible worldwide knowledge. I didn't have all the facts at my fingertips the way he did. I omitted a lot of topics, I'm sure, that he had covered very thoroughly. I just had to talk about things that I knew something about or was able to learn fast. I can't remember much about how I pirated his course, cannibalized it, you might say.

Riess: Did his course refer back to the California Indians in a way that you would no longer be doing?

Foster: Well, of course, his knowledge of the California Indians was unparalleled by anyone. His course on the Indians of California was packed with all kinds of data. We had to learn the names of all the tribes, all of the languages, all of the customs, including pit-roasting of adolescent girls among the Native Americans who lived along the lower Colorado River, which is suggestive of humoral medicine.

Riess: Pit-roasting of adolescent girls?

Foster: As I recall, the adolescent girls, after they had their first menses, were buried up to their necks in a pit of hot sand, which had been heated by a fire at the bottom. The burning ashes were covered with sand so the girls' bodies were thoroughly heated, but they were not burned. I'm sure that was to increase fertility,

because in humoral medicine, infertility is always explained in terms of a cold womb.

This heating of women illustrates the way anthropologists utilize the comparative method in analyzing data. Among Malaysians, for example, after childbirth a woman is placed on a stick bed over a low fire, where she lies for a week in order to keep her warm, so she won't suffer and so she'll be able to have other children. Sterility is explained in terms of a cold womb.

And in Latin America, at least in past years, post-parturient women were warned not to drink anything thermally or metaphorically cold, so that their wombs would not be chilled, which would make it difficult, and probably impossible, for them to conceive another infant. They were supposed to eat and drink only warm foods. It is evidence like that that leads me to believe that "pit-roasting" of girls in indigenous California was viewed as a health measure to ensure the fertility of the girls.

Riess: That's fascinating. Fertility is a great good. But how about control of frequency of conception? Might a woman choose cold in order not to have yet another child?

Foster: That's a good point, and I don't believe it has been investigated. I've never encountered it in Mexico in my work there. But I was interested--at the Philadelphia AAA meetings last December I became acquainted with a very interesting young anthropologist named Gwynne Jenkins, who had worked in Costa Rica.

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Foster: Gwynne Jenkins asked me if I had information as to whether in Mexico "the pill" had a metaphoric quality, and specifically if it were considered to be "cold." I said, "No, but I'll check when I am in Tzintzuntzan this Christmas, in a couple of weeks." It was a good question, because many modern medicines are assigned humoral qualities on the basis of their use. Aspirin, for example, generally is interpreted as "hot," probably because it is used for colds. So I suspect that the pill, because it prevents births, would be considered to be "cold," thus chilling the womb.

But when I asked several women friends in Tzintzuntzan they said that the pill was neither "hot" nor "cold" as far as they knew, and that the doctor had assured them that it would not interfere with any of their internal functions. Modern medicine has come so far in Mexico that most of the old beliefs are pretty well on the way out.

"Europe and the Mediterranean" Course

Riess: You said that when you taught "The Nature of Culture," you somehow compressed the Kroeber knowledge.

Foster: That was not a course I particularly liked. It was a course that had to be taught, and I taught it.

I was much happier with "Europe and the Mediterranean," which is one that I did introduce. I used the Mediterranean as an example of a culture area, which goes back to [Clark] Wissler in American anthropology--the idea that societies geographically adjacent to one another are generally organized around similar themes. The Mediterranean certainly seemed to me to be a distinctive culture area, because in all Spain, Portugal, Italy, France, Greece, the Near East, one found very similar beliefs and customs. That was a course I really enjoyed giving.

The seminar on "Acculturation" I liked very much, too, because that was something that I kind of grew up with. That is, I became an anthropologist at the time acculturation theory began to be developed in this country--it was different from the British, our acculturation theory. I was interested in it for a great many years. I still am, though it's not the central interest that it once was.

Riess: In "Europe and the Mediterranean," you're talking about those groups historically?

Foster: History and--I read everything I could find about religion and social organization, and found the godparenthood relationship, the *compadrazgo*, which you find all through the whole area--it's a combination of Arab and Catholic influence, I think, that gives the dominant characteristics to the European half of this area.

Riess: You didn't have to eliminate the present-day effect when you were talking about the whole cultural area of the Mediterranean?

Foster: I'm talking about a period almost fifty years ago. Like Mexico, the world has changed dramatically in those fifty years. I don't know how I would handle it today. It would be very different from the way I handled it then. I'm still handling it in standard anthropological terms, "standard" meaning the way I'd been trained, and my experiences during the fifteen years or so since I took my degree. "What is the historical background and how has that played out in the present, and how does it compare with similar societies?"

Riess: You had your own experience in Spain.

Foster: Yes, that was a very important part of the course.

Riess: Had there been work done in other Mediterranean countries that you could look to?

Foster: Not my kind of work, but there were accounts of fiestas and things like that, so I could get a lot of comparative data. I think the first American to work in Europe as an anthropologist was Conrad [M.] Arensberg at Columbia, who did a book on the Irish countryman, published about 1939.

In the Mediterranean area, I was not the first to publish, but I was the first person to do field work, the first American. Interestingly, Oscar Lewis had gone to Spain in the summer of '49. He was there for two or three months, but as far as I know, he never published anything. I'm sure he went for the same reason I did: because of his knowledge of Mexico and his realization that we had to know what was in Spain. But he got shunted off first into India, and then to Cuba, so I don't think he ever published on Spain.

So I really could say I was probably the first American anthropologist to work in Spain, and maybe to work in continental Europe on a major scale.

Aside on Oscar Lewis and on Motivation

Riess: Did you know Lewis well?

Foster: Quite well, yes.

Riess: Would he be someone you would review your ideas with? What kind of collegial relationship did you have?

Foster: I think I had a good relationship with him. I liked him, I respected his ability, I liked his work. Particularly he did a wonderful restudy of Tepoztlán, which was far and away the best restudy I think that's ever been done. That got him in hot water because he criticized Redfield's Rousseauian view of peasant society in Tepoztlán. He said, "It's not like this at all."

I had come to similar conclusions on the basis of Tzintzuntzan, that Redfield was wrong. So when Oscar was criticized roundly by a great many people, who said, "He's just

projecting his own personality," I took issue with them. That was why I ultimately developed the idea of "limited good" to try to explain the personality of Mexican peasants and peasants in general, economically deprived people.

Riess: Did he develop theories?

Foster: The "culture of poverty" is the theory he developed, and he was widely criticized for it. I think he was right, though. His idea was that people are born into a culture of poverty, and they learn, they grow up in that, and they have a terrible time getting out because they don't know anything else. I never understood why he was so criticized. It seemed to me it was a very valid idea, that one learns the behavior of the society into which one is born, and if your mother is on welfare and living from hand to mouth, and if you don't have a father around, or if you do he's a bum, and if you've grown up and been socialized in an atmosphere of poverty, it's very much a cultural conditioning.

Riess: Is this unthinkable for Americans? Because we come from such bootstrap values?

Foster: I don't know why it was. As I say, I've always been puzzled. It seems to me that it was a valid concept that helped me understand how you had one generation after another of people on welfare. It's not the only explanation, not a one hundred percent efficient explanation, but it's a good starting point in understanding welfare and poverty in our own society.

Riess: It could help people not be judgmental about poverty?

Foster: I suppose so. I never thought of it in those terms. I don't think anthropologists tend to think like that.

Riess: Anthropologists don't think in judgmental terms?

Foster: I don't believe they think in terms like that very much. We've been theoretically oriented, mostly, and our interest has been to understand and explain behavior. "Relativism" is one of the big terms we use. "Everything is relative," which is a poor way to state it. The concept is much abused, I think. If you're an extreme relativist, you've got to accept Hitler. You've got to say all cultural forms are valid if you understand their relative positions, which I think is a lot of malarkey. I've written that I think there are some cultures that need a general overhaul in the worst possible way. That is an extreme position among anthropologists.

Riess: Anthropologists must have to understand well the assumptions they've had from the cradle.

Foster: Yes. I think people go into anthropology, at least in my generation, because for some reason, whatever reason, they were rejecting many of these assumptions. They wanted to break out of their own bonds and get into a different environment, where one is exposed to different ideas. In my case it was a way of getting out of small-town, Midwestern society. I think probably Margaret Mead, in going to Samoa, wanted to break out of her somewhat Victorian background. I think that's true of most anthropologists. I don't know why it is, but the exotic has an appeal.

I've often thought that missionaries and anthropologists are a lot alike. Foreign missions, along with anthropology, were about the only way a person could break out of his own society. Anthropologists differ from missionaries in that we generally try not to take our beliefs with us; we try to leave them at home. But I think the personal drive is very much the same in many cases. Of course, historically, some of the best accounts of non-Western peoples have been written by missionaries in Africa and in Oceania.

Riess: Do you think this motivation holds true now, fifty years later?

Foster: No, because there are so many ways one can go abroad. Fifty years ago, forty years ago, thirty years ago, anthropologists I think were the most widely traveled university professors. Now, in the age of jets, with international conferences, it doesn't make any difference what your field is; we're all going abroad to meetings or taking a sabbatical year in Italy or Australia or someplace. It's quite different than it was when I was younger.

Riess: So, we were talking about your "Europe and the Mediterranean" class.

Foster: I don't believe that course is taught now, at least not in that fashion.

As you get more and more data and more and more professors, the geographical units for a course become smaller and smaller. We now have a course, I expect, on Spain alone. We used to give the ethnography of Latin America as a course; now, it could be Peru, or Mexico. We get more intensively involved, but we do it at the expense of the comparative view, which I think is too bad.

Low Points of 1958-1961, Chairmanship

Riess: You were department chair from 1958 to 1961?

Foster: Yes, and the low point in my chairmanship came in early June 1960. In the preceding years, we had taken on three people, very able in their own ways. One was David [M.] Schneider, who had taken his degree at Chicago and had been a junior appointee at Harvard. At that time, Harvard had a system of hiring two people: one would be kept on, one of them would be let go. And he was the one that was let go. He was not happy about it. He was out at the Center for Advanced Studies when Kroeber was there in 1956-1957.

We invited him up here once, and he gave a very interesting lecture. The more we learned about him, the more we thought he'd be an excellent person. I remember asking Kroeber on several occasions, "Should we hire Schneider?" Kroeber would never give an answer. He would only say, "He's a very tragic man." That's the only answer he'd ever give. I came to see what he meant, later. But we hired him, and I think Kroeber thought we did a good job in hiring him.

But he [Schneider] was a very strong personality, and one of the first Americans to become enamored of British social anthropology. And he felt that he had been hired to remake the department in the image of British social anthropology. He began teaching students that Kroeber and Lowie were old fogies, that the kind of anthropology they did--there was no point to that. That didn't sit very well with a lot of us.

The other two people that we hired were Lloyd ["Tom"] Fallers, who had come from Chicago where he had taken his degree with Fred Eggan, who at the retirement of Redfield became the dominant anthropologist at Chicago and was a major figure in anthropology. A wonderful person. Fred asked Tom Fallers to come back and help him with the department, and for the same reason that I would have come to Berkeley to help anyone who wanted me, Tom had this affection for the University of Chicago.

The third was Clifford [J.] Geertz, who was a young person we had hired. And I think he was not happy at Berkeley. So all three, in the middle of the spring of 1960, left to go to Chicago. And Dave [David] Mandelbaum, who became angry at me because I was not running the department the way he'd like it to be run, had a friend who was a reporter for the *Chronicle*, and they ran a story blasting me as chair because I made no attempt to keep Schneider. When Schneider said, "I'm going back to Chicago," I congratulated him. I didn't say, "Won't you stay?" Mandelbaum didn't like

Schneider, either, but he--so that's the one time I resigned, I got just so fed up.

It was a contentious department at best. If it hadn't been for Glenn Seaborg, who was then chancellor, and the dean, who was Bill [William B.] Fretter, I don't know what would have happened. They were both very supportive. Obviously they talked me into staying on as chair, although I can't find any letter in which I withdrew my resignation. Probably there was no such letter and it was all handled informally--but I am sure I wrote a letter in which I said I couldn't take it any more and was resigning. From this experience I learned that resigning, or threatening to resign, can be a useful administrative technique, if you don't use it too often. But at the time it had never occurred to me to use it as a way to gain administrative sympathy and support.

Riess: You were chair after only being in the department for three years. Who preceded you as chair?

Foster: Dave Mandelbaum had been chair.

Not everyone can be chair. There are people who have personalities that--they're bright and competent, but they just don't have the talent to be chair. I had had administrative experience at the Smithsonian. I guess they recognized it here, when I was running the museum, that I knew how to administrate. It didn't strike me as being unusual after being here for three years, to be picked as chairman. After all, they're advertising for an outsider to come and run the department now. It's just sad to think that the department has come to that point, where it can't provide for its own administration.

Riess: In some departments it's viewed as a thankless task.

Foster: It is. Nobody is ever remembered for being a good chairman while that person is chairman. It's only when you're faced with his successor that you realize the other guy had been pretty good.

Stanley Brandes, who is now chair, is the only one that I know that has been appreciated while he's chair. He said he'd be glad to do it again sometime in the future, but he's finishing three years now--maybe it's longer--and he said he just gets worn out. I appreciate that. It's a sacrifice. People who aren't chairman don't realize what a wearing experience it is. It gets so much more complex with every year, more rules and regulations. Now so much of a chair's time is devoted to meeting offers from other universities. That's a terrible thing.

It used to be that people would join a university and expect to stay there their whole life. Now they'll jump for a thousand dollars more salary. A lot of a chairman's time just goes into trying to keep people happy.

Riess: It sounds like Chicago was the other great anthropology department then? It was just Berkeley and Chicago at that point?

Foster: I think Chicago was *the* great department at that time, more than Berkeley.

They didn't use to rate departments the way they do now. Berkeley, when I decided to do graduate work, was considered one of the great departments. The only other ones were Chicago and--Columbia was still highly rated, much more highly rated than now. And Harvard was a good department, although it was the one that was not Boasian. It followed its own lines.

But then I'd say beginning about 1935, Chicago was the number one department. It has been at or near the top much longer than any other department, I'd say, and still probably would be rated tops by most people. About 1960 Berkeley began rapidly rising. For a number of years we were rated number one. We're at about number three now, I think, in these formal ratings, which is very good but not quite as good as it was.

Opportunity to Add Washburn, Clark, and Colson to Faculty

Foster: I think what catapulted us into the lead was not hiring assistant professors and hoping they'd turn out to be able. Instead we were hiring full professors who were forty-five or fifty years of age, who had already done good jobs, and it was clear that they were leaders, and they didn't rest on their oars after they came here. That is, we brought known, recognized talent--people like Desmond Clark and Sherry Washburn, Elizabeth Colson, to name several.

Washburn was the first star that we hired. He came about '57. Desmond Clark came about '61, and Elizabeth Colson in '64.

Riess: Do you remember talking about this as a policy?

Foster: No, we had no policy. We just picked--enrollment was growing rapidly, there was all kinds of money available in those days, so we always went for the highest level appointee we could get. We figured it was a good idea to get people that were well

established, if possible. I still think it's the best way to build a department rapidly.

Riess: This was the very end of Sproul's presidency?

Foster: After Clark [Kerr] took over. Sproul retired in June 1958.

Riess: Can you ascribe it in part to Clark Kerr?

Foster: I think only indirectly in that he was a fresh breath of air in the university. As I say, I ascribe it particularly to the fact that money was no obstacle in those days. We had money galore coming into the university, on a scale we've never had since, really.

Riess: Despite Kerr's eventual problems with the regents, the legislature was still being generous.

Foster: I think his problem was with the governor, with [Ronald] Reagan.

Riess: Did having stars on the faculty, like Clark, Washburn, and Colson, inspire division of the program into the various groups that are reflected in the catalog: Physical Anthropology, Social and Cultural, and so on?

Foster: No. We were not really divided into groups then. McCown was the first physical anthropologist. We had no linguist. Kroeber and Lowie were the department: Kroeber was first, Lowie came in 1917. [Ronald] Olson was a product of the department; he joined after he took his Ph.D. in 1929.

McCown was also a product of this department. He was our first physical-biological anthropologist. He was away most of the time while I was a graduate student. He was working with Sir Arthur Keith, writing the report on the Natufian skulls, a find of early man in Palestine that Keith had made. McCown had been his assistant, and when Sir Arthur fell ill McCown was given the responsibility of writing up the final report, as junior author, as I recall. So he came back to Berkeley as a star.

Unfortunately, like a number of people I have known, McCown achieved fame far too early for his own good. He had a reputation he felt he could never live up to, and it paralyzed his writing, and also his administration of the department. However, he brought Washburn into the department; he recognized that Washburn was probably the number one physical anthropologist in the country.

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Foster: He recommended Washburn, he hired him, and we agreed. Some of us knew him slightly through the AAA. He was active in the affairs of the association. He was very well known at that time. He was the best person we could have brought because almost immediately we began getting physical anthropology Ph.D. candidates. And Washburn played the major role in bringing Desmond Clark, because from the work he had done in Africa he had become acquainted with Clark, and it was clear that Clark was a real discovery, too.

Riess: Where were they trained?

Foster: Washburn had all his education at Harvard.

Clark was a Cambridge man. He's British. He had been in Africa, in northern Rhodesia at the Rhodes-Livingstone Museum at Victoria Falls for a number of years before and after the war. He immediately brought Glynn Isaac, who was a young South African. He was with us for a number of years, and helped us enormously until he was lured away by Harvard. Sadly he died two or three years after going to Harvard. He had been in China and had come down with some kind of bug and was never able to shake it. But he was a tremendous person.

An Expanding Field, New Scholarship, More Students

Riess: When I talk about various groups in the catalog, by 1966, there was Group I, Physical Anthropology and Primatology.

Foster: I think it's just a grouping of courses. But it's true that when they had several people in one field, they tended to get a group of students, as a group. In social anthropology we didn't do that so much. Though Schneider and Fallers and Geertz tended to do it, along with Erving Goffman in sociology. Goffman had a tremendous influence on anthropology at one time.

Riess: He was in the sociology department.

Foster: He was intellectually closer to Schneider and Fallers and to Geertz than they were to old-timers like me. He was a very interesting fellow. He went to Pennsylvania and died young also.

Riess: Did you have psychologists and sociologists lecture in the department?

Foster: George [A.] De Vos was the first person we had in what's now called psychological anthropology. Originally, it was called

culture and personality. This is a field that developed under [Abram] Kardiner and Ralph Linton in the late thirties and early forties. Cora Du Bois, who took her degree in 1932 here at Cal, was one of the principal ones.

George De Vos came to us from psychology, or sociology maybe--no, he was in the school of education, I believe. He had an M.A. in anthropology and a Ph.D. in psychology from Chicago. Our students began going to him for culture and personality, which is what we still called it. He asked to come into the department, and we decided to bring him in.

Riess: As chairman, or any time, was there a time when you felt that it was becoming unrecognizable as an anthropology department?

Foster: No, not when I was there. It was relatively simple, although it seemed complex. For example, as chair I did a great deal of the interviewing of candidates for assistant professorships. The story of hiring Laura Nader illustrates how simple things were in those days. I had met Laura in the spring of 1956 when I visited a seminar of Vogt [Evon Z. Vogt] at Harvard. She impressed me very favorably at that time, although she was not far along in her studies. The next time I met her was in Mexico City at the AAA annual meeting at Christmas, 1959. At that time I was greatly impressed by her account of her doctoral research in Oaxaca. After interviewing twenty or more other hopefuls I was convinced that she was the cream of the crop among young anthropologists who would be in the job market for the next year.

I came back to the department, and I said, "I think Laura is the best anthropologist who is getting a degree this year. I recommend we hire her." That's all we had to do. We didn't have to advertise the job. We weren't thinking about women versus men, gender. I just said, "I think she's the most promising anthropologist this year."

They said, "Well, if you think she's the best, fine." So that's all there was to it. We just wrote her a letter and said, "We'd like you to come."

A year or so later, while I was still chairman, we brought Gene [Eugene] Hammel back. He had taken his degree here and had gone to New Mexico. We had kept our eye on him, but thought it wouldn't hurt him to get a little more experience in another environment before inviting him back to Berkeley. When we learned that Harvard was angling for him, we knew we had to jump fast, so we grabbed him. He preferred to come here to Harvard--which I would have also preferred myself!

Riess: When you talk about "we" and jumping fast, who was this group?

Foster: All of the tenured professors. There weren't very many, a dozen or so. But we were a committee of the whole, and we decided on whom we would invite to join our department in staff meetings. I remember that at an annual meeting of the AAA when I was graduate advisor I had spent a good deal of time interviewing candidates, so I knew the ropes pretty well.

Riess: Did Laura Nader come out and give a talk and get looked over?

Foster: No. There wasn't much money to bring people in those days; although I speak about the money we had, I don't think we brought any of them. [Gerald D.] Berreman is another one I remember that I hired. I must have met him at one of the annual meetings.

The annual meetings were terribly wearing for me, because I spent almost all of my time interviewing hopefuls for jobs here. After the first two minutes talking to the candidate, you knew if the candidate was a realistic possibility, but I had to go through the ritual of at least a half an hour with all of them. After I'd interviewed thirty or so candidates, I felt as if I had been put through the wringer.

Riess: That would be just when you were chairman, or was that always something you would do at annual meetings?

Foster: We now have committees that interview students. Ever since my chairmanship we have always interviewed people at the annual meetings. But we hired so few people before then, there was no need to send people to interview them.

Riess: What was propelling the need for all these people in anthropology?

Foster: The field that was growing rapidly. But the whole university was expanding. I don't know how many students there were in 1955, but I'd guess fewer than half the number we have now. Very quickly, in the next fifteen years, it went up to thirty-two or thirty-three thousand. Clark Kerr, you'll remember, tried to ease the situation by have four equal quarters of teaching a year--the "multiversity," I believe it was called. Within ten or fifteen years, if I remember correctly, the university enrollment doubled. I believe that by 1970 we had reached the largest number of FTEs [Full Time Employees, i.e., staff members] we've ever had--certainly more than now.

Riess: And anthropology itself?

Foster: Around the country it has mushroomed unbelievably.

Riess: Why is that?

Foster: Well, I think it speaks to real problems. The concept of culture and culture shock and cultural differences. We hear of corporate cultures now, bureaucratic cultures. I think anthropology simply was a very popular subject. It's interesting intrinsically, and if you remember that it includes pre-history--people are always interested in the first forms of humanity that appeared on earth, and archaeology, much more so than they are in social anthropology, although there are more social anthropologists than there are pre-historians.

I think that by then a lot of high school teachers had had a course in anthropology, and social studies unquestionably talked about it. Before the war, very few people in this country could even give you a definition of anthropology. I think by 1955 probably all university people knew what anthropology was about, or at least had a good idea.

Riess: Was the G.I. Bill population maybe particularly choosing anthropology?

Foster: Yes. That was a major impact in all university life. These people who were past the age of frivolity, serious students, and awfully good students, wanting to get ahead rapidly, working their tails off. They brought the money with them, of course. That was the beginning of the money.

Riess: [reading catalog] Archaeology, Pre-History and Culture History were another grouping.

Foster: I think that's just some editor's idea. I wouldn't make anything of that, I think it's a blind alley.

Riess: All right. [laughing] Area courses was a new wrinkle, wasn't it?

Foster: No. We had always had area courses--Lowie's 101A-101B, Ethnography of the World, that was something everyone took when I was a graduate student.

Riess: But there are fifteen different areas now.

Foster: We have fifteen more professors than we had in those days. That's what I say. Courses used to be continent-wide, covering the whole world in two semesters. Now we'd need five years of country courses to do that.

National Institute for General Medical Sciences Grant

Riess: You were chair from '58 to '61 and then again in '73-'74. Why?

Foster: I swore I'd never be chair again, I was six years from retirement, I thought I had done my service. But there was no one willing to do it, and I remember sitting in staff meeting and hearing myself say, "I never thought I'd say this, but if somebody will take over for six weeks, when I'm committed to go to Indonesia, I'll be chair this next year." I remember Gene Hammel jumped on it right away. He said, "I'll take it for six weeks." They were so glad to get somebody to take it!

Riess: Was that a troubling year?

Foster: No. I don't remember anything difficult particularly. First of all, it was only for three quarters, and Gene spelled me for six weeks.

I was in charge of this huge grant we had from the National Institute of General Medical Sciences, which brought in almost \$3 million for graduate students in general. We had three five-year grants in succession. I learned about the National Institute for General Medical Sciences--the National Institute for Mental Health, NIMH, was the principal unit to which anthropologists made applications for field research. The National Institute for General Medical Sciences, another unit of the National Institute for Health, set up a program to support graduate anthropology, on the assumption that all anthropology was related or potentially related to health, and that if they supported the training of graduate anthropologists, enough would go into health to make it a good investment.

For some reason, I was asked to be a member of the first site visit in 1964 at the University of Minnesota. That's when I learned about the program. We were site visiting Minneapolis, where Ad [E. Adamson] Hoebel had put in for a grant. He found out about it somehow.

Riess: Who?

Foster: [E.] Adamson Hoebel. He had been president of the Triple A and was a very eminent anthropologist in Minnesota all his career.

Well, to make a long story short, those of us that were asked to be on this first site visit obviously had been picked from major departments, in the hope that we would come in with big requests because when you get money to give out, you've got to

have good candidates. I knew a good thing when I saw it, and I had a request in rapidly. We had one of the first grants following Ad Hoebel's.

I was on the committee that visited the sites for the next ten years, so I was in a position to know what was going on. And that's why we started the medical anthropology program, one of the reasons. It was clear that initially the grant was to biological anthropologists, social anthropologists, anyone--on the grounds that all anthropology had health implications. But as time went on, it became clear that they were going to be more and more selective and pick students and programs that emphasized health and taught health problems.

I remember the University of Chicago had one of the early grants. We thought they were rather conceited. They just said, "We're not going to change anything," as a result of which they lost a lot of money. So we started medical anthropology, in part to keep the money coming in, and in part to make it possible for the small medical anthropology group at UCSF to do graduate teaching and to give Ph.D. degrees, so that they could attract top-quality students. Those were both factors that led to the establishment of the medical anthropology program in 1973.

Riess: And you were director of the program until you retired?

Foster: I was director part of the time, and Margaret Clark, who had been my first Ph.D. candidate when I came back to Berkeley, was running the program in San Francisco.

Riess: It's really interesting that that whole program came about because of the money, the available money.

Foster: Well, money talks. As I say, I brought in three million dollars. I think we had almost a hundred students that were supported entirely or partially toward taking a Ph.D. on the basis of this money. [laughs] Life for graduate students has never been the same since. The money ran out in '79. That's one reason why I retired in '79. Gerry [Geraldine F.] Moos, who had been our secretary for the program for the whole time, had to leave because we had no money for her. And I retired two years before compulsory retirement.

Riess: The money ran out for medical anthropology or the money ran out, period?

Foster: Well, for medical anthropology, this grant, which the medical anthropology candidates were getting the bulk of--not all of it, but they were getting the bulk of it. And Gerry in a sense was my secretary, to the extent that I ever had a secretary. It just

seemed like a good time to let somebody else begin to worry about it.

I was doing a lot of consulting then, for WHO [World Health Organization] largely. I liked to be away and be able to go on the spur of the moment. So I was ready to retire.

Riess: And you didn't miss teaching?

Foster: No, oddly enough, I didn't. I came back about 1983 or 1984 to give a course called "Medical Anthropology," which I had never given, at least under that title, before I retired. But that's the only time I taught after I retired. I had a number of students that I had to work with, finishing their degrees. I guess for the next five years I had contact with students, and then since then it has been very sporadic, occasionally a bit of contact with a student.

Foster's Students in the Field, and More on Culture Shock

Riess: In the Kroeber Anthropological Papers [1979], "Will The Real George Foster Please Stand Up," it says about you, "It is noteworthy that he was the first social anthropologist in the Berkeley department to take students with him to the field, involving them directly in his own Mexican field work."

Why didn't more professors do that? What does that mean?

Foster: I'd had the--Kroeber had bits of money to send graduate students to the California Indian tribes, to fill in gaps that he had not filled in himself. In 1937 he gave me a couple of hundred dollars to spend the summer with the Yuki Indians. I realized that made it possible for me to jump in in Mexico and do my doctoral research much more efficiently than I could possibly have done it if I hadn't had that experience.

I had been on the staff only about three years when I began to think seriously about taking students in the field. I've mentioned the very bright young woman who took her qualifying exams and went off to Oceania, and almost finished her field work, but who cracked up in the Pacific. When she came back and I said, "You had culture shock, every student has it," she looked at me kind of reproachfully and said, "Why didn't you tell me that before I went to the field?" It hit me awfully hard. I thought, we've got to do more than just tell them they're going to have a tough time, that it's not all beer and skittles.

Cynthia Nelson registered for graduate work in the fall of 1958. Sometime during the winter she expressed interest in Mexico. In talking with her I got the idea, Why don't I take several students with me to the Pátzcuaro area this coming summer? I was about to renew my study of Tzintzuntzan after an absence of twelve years, and it seemed like a good time to try the experiment. I asked for money from the National Science Foundation and received a grant of \$41,000 to support several students, including Cynthia. It worked so well that for a number of years, although I didn't run a formal field school, I would take several students with me. [See further discussion, p. 271.]

Riess: What does that mean? Formal field school.

Foster: Well, about the same time, Evon [Z.] Vogt at Harvard had a big grant from one of the foundations, in cooperation with Joe Casagrande at Illinois, and somebody at Columbia. They'd take a number of undergraduates and graduates to the field in Chiapas and run classes with regular credit. They did wonderful work.

But I didn't like that. One, the scale was too big for me. And two, I thought it was time that graduate students should be on their own but have someone to whom they could come for help when they needed it. So my plan--which I thought was a good one, and I still think it so--was to take the students with me to the Pátzcuaro area, decide on villages where they could stay, help them find families they could live with, and then turn them loose and agree to meet them once a week either in Tzintzuntzan or in Pátzcuaro.

We could get together in between for a meal if we wanted to, but one afternoon a week, or one day a week, Friday usually, we'd meet in Pátzcuaro--that's the day of the market, and that's always exciting--and we'd eat together and everyone would report on the work that they had been doing. We'd comment on each other's work. It was a wonderful learning experience for all of us. I think they all profited enormously from that experience--although not all of them turned out to be great anthropologists, some of them dropped out.

Stanley Brandes and Van [Robert V.] Kemper subsequently went with me in 1967. They were among the later ones. They were two of the best. Cynthia Nelson, of course, did very well, as did May Diaz. May--she was a mature person when she came to me. She had been Schneider's student, oddly enough. When he left, since her interest was in Mexico she turned to me, and she did an awfully good study of potters in Tonalá, a suburb of Guadalajara, adjacent to Tlaquepaque. [Published by the UC Press in 1966 as *Tonalá: Conservatism, Responsibility, and Authority in a Mexican Town.*]

Theron Nunez, like May, was not a member of the Pátzcuaro group. He had decided to study a village called Cajititlán, twenty miles south of Guadalajara, which had a small lake that attracted many people from Guadalajara for weekends. I visited him several times while he was doing his research--as I did May. But for reasons of distance--it was about 180 miles to Pátzcuaro--they were never able to participate in our weekly gatherings.

Riess: About the culture shock, did you talk about it in classroom lectures?

Foster: There's this mysticism about the field experience, that it's the culminating experience of anthropology. It gets reified to the extent that we always thought, Gee, if we could just get to the field, everything will be hunky-dory, just a wonderful experience. We get to the field, and we find we're depressed, we know we can't do it.

I came back twice from the Yuki to tell Kroeber I couldn't make the grade. He encouraged me to go back. He said, "This is not your doctoral dissertation. This is just to give you a chance to do a little field work and learn how to do it." This encouragement meant that when I was with the Popoluca I had no problems at all.

Riess: As long as a person recognizes that he is in culture shock, it doesn't defeat him?

Foster: Usually it doesn't. Occasionally, even though you recognize it, you can't cope with the problem. But it just makes all the difference in the world if you know, "I'm not unique in this world in feeling I can't do the job, every other anthropologist has felt the same way." If you know you're going to get over it, and particularly--and I think this is very important--if you've got some older, more experienced person on whose shoulder you can put your head and cry, it helps a lot.

Riess: Maybe one of the natives in the village who has seen so many anthropologists can reassure you.

Foster: I hadn't seen many at that time.

Riess: Another course you developed was "Anthropology in Modern Life."

Foster: That was applied anthropology. Somebody, I believe it was Adan Treganza at San Francisco State, said, "If you call a course 'Applied Anthropology' nobody will take it. If you call it something like 'Applied Anthropology in Modern Life' you'll have them beating your door down." So I thought the first time I gave

it, Well, I'll take a chance on having the door beaten down.
[laughing] "Europe and the Mediterranean" doesn't produce that many students.

That was the most successful course, I suppose, that I ever gave. I mentioned earlier that I wrote two books on the basis of my lectures, *Traditional Cultures and the Impact of Technological Change* (1962) and then, of course, I had to rewrite my lectures. That gave rise to *Applied Anthropology*, after which I had to rewrite my lectures once more. And that resulted in the revised edition of *Traditional Cultures* that appeared in 1972, under the mercifully shortened title of *Traditional Societies and Technological Change*.

Lessons from Students

Riess: Tell me more about your public health lectures, what you were teaching there.

Foster: I think I told you that Bill Griffiths and Dorothy Nyswander were the people I made contact with in the School of Public Health when I came back to Berkeley. I liked them immensely, and they seemed to like me, and asked me if I would give a seminar, which I did. Fall of 1954 must have been the first one.

That was very interesting because I had students in health education not only from the United States but from several other countries. I particularly remember Dr. V. ("Ram") Ramakrishna, from India, who was president of the student body at the School of Public Health when I first knew him. Our families have been good friends ever since. His daughter, Jayashree, completed her Ph.D. under my supervision twenty-five years later. The year the anthropology library was named after Mickie and me (1997) Ram received the Haas International Award, an honor conferred annually upon former students from other countries recognized for having done exceptional work in their fields. Ram, who is just my age, had a distinguished career in India as a health educator. He held several high posts in his country, a major figure, an awfully nice fellow.

Working with these students, and getting their stories, gave me all kinds of vignettes. For example, I got the idea of teaching them how I did field work interviews, so I'd call for volunteers. We'd sit down, and I'd ask the student about some aspect of diet or something. I remember one session: I thought I was doing very well, I was questioning a Korean. When we were all done, he said, "That was very interesting for me, but I should

tell you, to be perfectly honest, if you had been interviewing me in my own country you wouldn't have gotten anything at all."

I was astonished, and so were many of the class. I said, "Why?" He said, "Because you looked me in the eye continually." I realized that that's what I had learned in Latin America. He said, "In Asia, that's bad manners." You look at the person in the face for a minute, then you look up and you look out the window and then you look back--. I remember one of the students from Chile put up her hand and said, "But you did that absolutely right from my point of view." I thought that was a wonderful experience. If I could get students to volunteer things like this it was very successful teaching.

Riess: These issues of how to behave, these matters of manners and customs, is that in the province of anthropology?

Foster: Very much so, yes. The fact that in Polynesia and many places you keep your head below that of the chief, and you don't sit with your feet pointing toward anyone on the floor---all kinds of things that anthropologists routinely learn come as a great surprise to other people.

Riess: Has anyone codified it?

Foster: A lot of people have codified it. Ed Hall, Edward T. Hall, in language has done a great deal of good work.

Comments on Redfield's Work in Mexico

Riess: Did field work in Mexico and Central America become eventually like California Indian field work, in that it was the logical, closest direction to go.

Foster: Yes. It was easier to get there than any other foreign country, other than Canada. It was the closest place you could go and find real societies as going concerns, other than the Southwest, which had had more anthropologists than they should have had for many generations, you might say.

Riess: You mean it was detrimental?

Foster: Yes. It got to the point where a Zuni or a Hopi family was described as the father and mother and three children and one anthropologist. That was kind of a sick joke of anthropologists.

But Mexico--Redfield was the first American anthropologist to do really serious work in Mexico, beginning in 1926 in Tepoztlán. His book kind of burst as a bombshell. We just hadn't thought of Mexico before that as being a place to do research. And Ralph Beals, who preceded me here at Berkeley by a few years--ten years or so--and Carl Sauer in the geography department in the early thirties began doing research in Mexico. So when I told Kroeber that I wanted to work in Mexico, after I made my first trip in '36 to Mexico, there was some precedent. Isabel Kelly, who took her degree in '32, was strongly influenced by Sauer and lived in Mexico at the time. She did both archaeology and social anthropology on the west coast of Mexico, and the social anthropology of the Totonac in Veracruz.

But very little field work had really been done by Americans before 1940 or so. Morris Swadesh and Norman McQuown had begun linguistic research in Mexico about 1940, and Sol Tax had worked with Mexican and Guatemalan students in Chiapas, about 1941, although the bulk of his research was done in Guatemala, beginning about 1935, under Redfield's guidance. Elsie Clews Parsons began the research for *Mitla: Town of the Souls*, in 1929 and published the book in 1936.

Riess: Before we finish today, you mentioned Redfield and I want to find out more about Redfield's Rousseauian view. Was it a view of Mexico as an unspoiled paradise?

Foster: Yes. He painted the picture of a Mexican village as being harmonious, natural, free of the vices of the city. It was as pure as Rousseau. At the same time, both Oscar Lewis and I concluded on the basis of our research that peasants were both very wonderful people, and very snotty people at the same time. Many would sell their grandmother, if necessary, to get ahead.

Riess: Snotty?

Foster: Unpleasant to each other.

Riess: Did you discuss any of this with Redfield?

Foster: No, I never did with Redfield. I had very little contact with him. He was at our house once in Berkeley when we lived up on Kentucky Avenue, about 1957. He rarely came to anthropology meetings. If he did, he'd come and deliver a paper and disappear immediately. I never really felt that I knew him very well, as I did other anthropologists. We were all puzzled by Redfield's Rousseauian portrayal of peasant life.

As Oscar Lewis has pointed out to me, Redfield had had to cut short his research after eight months because there was a revolution and he couldn't stay on. That didn't sound as if they were very peaceful.

Riess: [laughing] You mean a revolution in the village?

Foster: Yes. It was unsafe for anyone to be there.

This view, of course, goes back to the ancient Greeks. All the Greek classical authors painted a picture of peasant life as the ideal, contemplative life, as against the city life. Rousseau was not the first by any means, but he epitomizes this view. You find it all through the classical literature. The city is evil, artificial. The country is beautiful--life is simple, harmonious, close to nature. Thomas Jefferson had the same idea--he wanted to keep the United States an agricultural country. We would exchange American agricultural products for European manufactured goods, thus avoiding the evils inherent in urban centers; if we could avoid these urban evils we'd maintain our virtues.

Redfield was simply the last or the latest incumbent to hold that position. He was much influenced by the Chicago school of sociology. His father-in-law was Robert Park, a major sociologist, along with [Louis] Wirth. (Margaret Park was Redfield's wife.) He, Ogburn, and Louis Wirth were members of the Chicago school of sociology, which is urban sociology. They studied urban life with all of its obvious problems, and I think they're the ones who contributed much to Redfield's position.

It's interesting how you go to the field with a presupposition, and you find what you expect to find. That's what Redfield expected to find, and he found it.

Riess: Have you had instances of that?

Foster: I don't know. I don't think ego is ever in the position to answer that very well, I think you'd have to ask some of my colleagues. If you ask my socialist colleagues, they'll say, I'm sure, I certainly see it as a capitalist, life in villages, as well as in cities. I hope I have not been as egregiously wrong as Redfield.

Corresponding with Students in the Field

[Interview 7: February 10, 1999] ##

Riess: I talked with Stanley Brandes yesterday, and he said that one of many remarkable things about you was that you answered letters from the field from students who weren't even your students, that you were one of the people to whom people could write.

Foster: Yes, because when I was a student here, and for the first ten years after I was teaching, when I came back, I didn't have the idea that the professor who chaired the dissertation was the only person who was really concerned for the student. To this day, I don't know whether I was Kroeber's or Lowie's student. That's amazing. I find others in my age group at that time didn't know whether they were Kroeber's or Lowie's student, either.

When I came back to Berkeley, for the first six or seven years I assumed I was there to help anyone who wanted help, whom I thought I could help. It's quite different now. It's inevitable, I think, that we now have to have our own students. I think that doesn't mean we can't help others. I have always helped other students.

Riess: Is there an issue of trespassing, if you become close to another professor's students?

Foster: I never had that feeling. I don't know how it is today. My intimate knowledge stops in '79, of course, when I retired. But I don't think we felt we were trespassing. No one was hiding anything from anyone else. I was always delighted when I had help from other people with my students. I don't recall there were problems of that type, though I may see things through rose-colored glasses after twenty years.

VIII LATE 1960S, EARLY 1970S, AND THE AMERICAN ANTHROPOLOGICAL ASSOCIATION

The Free Speech Movement at Berkeley

Riess: How did you respond to the Free Speech Movement and the various political movements on campus in the sixties?

Foster: I was not sympathetic to it. I had some students who were-- Richard Currier, I remember, was a very bright fellow. He helped me a great deal as my Berkeley research assistant on Tzintzuntzan, and he published an excellent article on humoral medicine in Erongarícuaro. He was a very ardent supporter of the Free Speech Movement. I remember he and his wife, who was pregnant at the time, were arrested in front of the St. Francis Hotel [San Francisco], about 1964, I think. He was very angry at the police for arresting his wife, because she was pregnant. I thought, What business does she have, being pregnant, being over there? But we got along fine.

He paid me the compliment of telling me later on--he said, "I remember what you said. You said you wouldn't have done it yourself, but you admired me for doing it and respected me for doing it." I like to think that was my view, that I can disagree with people and not be angry at them--as long as I think their integrity is there. There have been cases where I felt they were lacking integrity, and my anger has been very great on those occasions. I won't mention names.

We had the good luck--or the bad luck--to be away during many of the rough periods. I was on sabbatical for the academic year 1964-65, and again during the winter and spring quarters of 1968. And during the last of the Free Speech Movement we were at the Center for Advanced Study in Behavioral Sciences in Palo Alto, where we also had problems.

The AAA Ethics Committee, and Anthropology on the Warpath

Riess: In the video interview with Charles Wagley you talked about "the volatile period of the late sixties," and you said to Wagley that you were happy to block it out.

Foster: I was indeed. But before going into the particulars, I think a bit of background will make the matter clearer: In those days the president and president elect of the AAA took office just before the business meeting of the annual meeting, held just before or after Thanksgiving. This meant that the first official act of the new president was to preside at the business meeting, and try to cope with all of the problems that had originated in the past year. I took office as president-elect at the Seattle meeting the week before Thanksgiving, 1968, under Cora Du Bois as president. I took office as president a year later, 1969, at the New Orleans meeting, with Chuck [Charles] Wagley coming in as president-elect in San Diego in November of 1970.

Riess: Now I know that there is another story in here, about the ethics committee and "Anthropology on the Warpath."

Foster: That's a complicated story. The whole association nearly blew up over the matter. Again, a bit of background is in order. The AAA at that time had a simple administrative structure: a president, a president-elect, and six executive board members, in addition to an executive secretary who ran the Washington office. During most of my year as president of the AAA--November 1969 to November 1970--the executive board members were Gene Hammel, a source of great strength to me; Dell Hymes, who had been on the Berkeley faculty, a good friend; Dave Schneider, Dave Aberle, Jim Gibbs and Cyril Belshaw. At that time the association was just beginning to face up to the question of ethics, and we had appointed an ethics committee, the chair of which was Eric Wolf. A second prominent member was a fellow named Joe Jorgensen.

Eric Wolf was a very eminent anthropologist--certainly, no better anthropologist in the country--a few years younger than I am. We had both worked in Mexico. Our careers have been quite similar in that we've been interested in Mexican peasants and the development of Mexico. His interpretation is Marxist. He was born in Vienna and came to this country as a young man. So he's American in every respect other than his birthplace, Vienna.

The ethics committee had been given a single charge: "recommend to the board what you think the role of the ethics committee should be." The ethics committee was thus a committee in formation, with no authority as yet to make pronouncements on ethical matters.

Riess: Had there been a particular felt need?

Foster: Ethics in anthropology had been largely a question of how do you relate to the people you're studying. The Society for Applied Anthropology had had a statement on ethics since about 1950. The Triple A, I believe, had a statement on ethics, too, but we had not had a committee to consider charges that people might bring up against the association or against individual anthropologists.

I think the Free Speech Movement and everything that went on in the sixties led us to see that the time was coming when we were going to have to have a mechanism, a more formal mechanism than we had had, so we appointed the committee. But their only charge at that time was to report back at the next meeting and let us know what they thought their role should be, and then we would finalize their terms of reference.

Riess: And what happened?

Foster: What happened was that in the spring of 1970 an eminent anthropologist's files, containing information about his role as consultant to the United States government about Vietnam policy, were Xeroxed by his research assistant, and she gave the copies to Wolf and Jorgensen. As a result, for a while I thought the whole organization was going to go to pieces.

Riess: Yesterday I looked at the *New York Review of Books* correspondence, so I think I know something of what you're talking about. I read "Anthropology on the Warpath: An Exchange."¹ That must have been painful.

Foster: That was just hell. I remember I got a call about six o'clock in the morning at home--I wasn't even up yet--from the *New York Times*, asking me what I thought about the matter. I didn't even know what they were talking about, but I found out pretty quickly.

I don't know how much you want to go into it.

Riess: I think that the oral history is a good place to go into it.

Foster: Well, Michael Moerman at UCLA was involved in advising the government in the Vietnam debacle in Southeast Asia. So were Lauriston Sharp at Cornell and Her[bert P.] Phillips of our

¹"Anthropology on the Warpath: An Exchange," *New York Review of Books*, April 8, 1971. Foster letter, pp. 43-44; Peter Hinton letter, p. 44; A.J.F. Köbben letter, pp. 44-45; Joseph Jorgensen and Eric Wolf reply, pp. 45-46.

department. There were two or three others--I can't remember, I should have reviewed my notes on this.

There was a national group of students calling themselves the Student Mobilization Committee, violent in its opposition to U.S. involvement in Vietnam. I would have described them as a group of firebrands. Others might describe them otherwise. Michael Moerman had one of the members of this group as his research assistant, with access to his files. She Xeroxed a whole passel of notes, just page after page after page, documenting his advise to our government about how it should deal with the Vietnamese. She, or the committee [Student Mobilization Committee], passed this information on to Eric Wolf and Joe Jorgensen who, speaking in the name of the [ethics] committee and the association [AAA], said that this is unconscionable behavior, that these people should be censored.

I was shocked because 1) I was surprised that they would do it, it was not their charge, and 2) speaking in the name of the association without authorization seemed to me to be deplorable. And I said so. But it was the temper of the times. Laurie Sharp was one of the nicest and most ethical people I had ever known. He visited here in Berkeley, lived in our house for six months when we were on leave once. He and his wife were top-notch people.

Moerman I didn't know well, but I think they were all highly ethical people, and to charge them with betrayal of the people of Vietnam and cozying up to a government that was not popular seemed to me to be completely unethical. And it was difficult for the board to act because Dave Schneider was a colleague of Jorgensen and a close friend of Eric Wolf, and supported them heartily. Dave Aberle, to a considerable extent, did the same. So we couldn't--if I had been in charge completely, I would have canned them both from the committee. But the board simply ended up giving them a slight slap on the wrist, saying they should not have done this since they were not authorized to do it, and not to do it again.

That angered them, and they both resigned from the ethics committee, and for the next eighteen months the association was in a terrible flap. "Anthropology on the Warpath" was the statement that Jorgenson and Wolf prepared, but I've not read it for a long, long time.²

²Eric Wolf and Joseph Jorgenson, "Anthropology on the Warpath in Thailand," *New York Review of Books*, September 19, 1970, pp. 26-35.

Riess: I didn't read it. That issue of the *New York Review of Books* had disappeared from the library. What I've read is your response, and then Jorgenson's and Wolf's response to you. Did this destroy careers? Moerman's and Sharp's and Phillips's?

Foster: Sharp was near retirement, but it crushed him. It probably hastened his death. It split the association right down the middle. Many of the older people were strongly opposed, and the younger people were very much in favor of it. You know my position there from my letter.

I was sent a Xeroxed set of the purloined files, so voluminous that they fill a file cabinet drawer. The file is badly reproduced and hard to read. I wondered how anyone could have read all of it in the space of a few hours and come out and make the accusations that Wolf and Jorgenson made. The "evidence" should have been carefully examined for days before drawing conclusions. Wolf and Jorgenson must have made up their minds before they examined the documents carefully.

Riess: Eric Wolf was a colleague, not far from your age, and you would not have expected this of him?

Foster: That's right. I didn't.

Riess: What would have become of the whole issue of the ethics committee if, in fact, they had never been given this pile of papers?

Foster: It would have been given a charge to deal with things like this, legally. It was the illegal aspect that bugged me: they were not authorized to make any public pronouncements about ethical matters. As I say in my letter, if they had spoken as individuals I think it would have been questionable but acceptable. But to identify themselves, speaking as members of the ethics committee, that gave the impression they were speaking officially for the association, which was absolutely untrue, of course.

Riess: Did they need to resign, or could this have been resolved as gentlemen and scholars?

Foster: I believe that the only course open to them was to resign which, of course, is what they did. To my way of thinking, their action was a violation of the basic rules of civilized intercourse between groups and individuals.

Riess: And you say this split the association?

Foster: For a while I wondered if the association was going to go to pieces.

Another matter that was like pouring oil on a blaze was the election for president to follow Wagley, which took place by mail ballot several weeks before the San Diego meeting. The AAA nominations committee had nominated Tony Wallace [Anthony F.C. Wallace], Jim Spuhler [James N. Spuhler] and Al Spalding [Albert C. Spalding] as candidates for the presidency. The AAA election rules also specified that additional candidates could be nominated if supported by a certain number of association members. I don't recall that this procedure had ever actually been used.

This year it was different. All three official candidates were highly qualified men, moderate in their views, certainly not radical. So Gerry Berreman [Gerald D. Berreman] of our department, a firebrand in those days, was nominated by a group of anthropologists sympathetic to the aims of the Student Mobilization Committee. Dave Schneider also backed Berreman. I was very angry at Schneider; I thought it was treacherous, to accept the board's candidates and then turn against them.

Berreman unquestionably would have been elected in a four-way election that would have split the majority vote among three very able candidates. But Spuhler and Spalding, recognizing the reality of the situation, made what I know were very difficult personal decisions, and withdrew from the race, leaving Wallace and Berreman as the only two contenders. And because Wallace was much better known, and represented the views of a majority of the association members, he won.

Riess: What next about the ethics committee affair?

Foster: The San Diego business meeting failed to satisfy the activist faction in the association. So the board, chaired by Wagley for the first time, following the business meeting prevailed upon Margaret Mead to chair a small committee to investigate the ethics committee affair, and especially the roles of Wolf and Jorgensen, and to report back a year later at the annual meetings, to be held in New York City. Her committee's report, which she presented orally, was critical of the young woman who had Xeroxed Moerman's data, and of Wolf and Jorgensen.

All hell busted loose. She was booed up and down by possibly a majority of the people at the meeting. She was astonished, and we were astonished--embarrassed too. It was just shameful, I thought. But as matters turned out, the blood-letting and booing of that night drained the venom from the veins of even the most ardent attackers. That was the end of everything: the war was over, the movement collapsed, and people lost interest. I thought we were going to have a whole decade of strife, but that last evening of booing and getting it out of their systems took

all the steam out of the whole thing. Amazing. The air was cleared, and we got back to the business of being anthropologists.

There was a historian, Eric Wakin, who wrote quite an interesting book on the whole thing. *Anthropology Goes to War* was the title.³ But I don't think most students in anthropology today know anything about it.

Getting Through the New Orleans Meeting, 1969

Riess: You and Wagley also talked on the video about the New Orleans meeting.

Foster: Yes. The ethics committee business was in the forefront in 1970, at the meeting in San Diego. In 1969 in New Orleans the problems were of a different nature. The issue there was the general high level of antagonism felt by students (and a few junior faculty personnel) toward all authority, including the governing organization of the Triple A. Student unrest in most universities was continuing to build up during this year, but I remember no specific events. However, by late summer 1969 I realized I would be facing problems that we had largely avoided until then, and I anticipated a rough baptism as president when I took over at the New Orleans meeting in November, with Chuck Wagley coming in as president-elect.

There were a number of people in the audience who were just set to raise hell. I opened the meeting by saying I was prepared to stay as long as anyone wanted to stay. There were no limits to what we could say. Say what you felt like it. Just try and be civil.

Fortunately for me the AAA recently had appointed a professional parliamentarian, without whose help I would have been lost. I had read *Robert's Rules of Order*. The sociologists had met in San Francisco a couple of months before we had that meeting, and I knew they were in trouble, so I went over--I was a member of the society--I went over and went to their annual meeting to get tips on how I would cope with my problem. I read *Robert's Rules of Order* very, very carefully many times, but you just can't memorize it.

³Eric Wakin, *Anthropology Goes to War: Professional Ethics and Counterinsurgency in Vietnam*, University of Wisconsin, Madison, Center for Southeast Asian Studies, 1992.

But I did come to an interesting conclusion: I concluded that *Robert's Rules of Order* is one of the two really great social inventions, along with double-entry bookkeeping. I've never heard anyone else mention this. I'd like to know. I'm sure other people have thought the same thing. The device that makes it possible for people, passionate people with vastly different ideas, to come together and debate with some semblance of order terribly difficult problems, and come to a conclusion, and then vote and abide by the rules. I've often thought what a remarkable invention it was. We couldn't have had democracy without it.

Riess: So you did stay as long as they wanted, and it got talked through in some way?

Foster: Well, everybody got tired, and little by little people would slip away. At the last, there were about a half a dozen of the firebrands, and they too began to get tired. Finally, about 1:30, I turned to the parliamentarian and said, "I don't think we have a quorum, do we?" And he said, "No." So I said, "I'm sorry. We'll meet tomorrow. There's no quorum." They were happy to leave, too. I think it was important to let people blow off their steam.

I got through that better than I thought I would. I'm always terrified when I'm on the floor. I'm very reluctant to stand up and make a comment, but when I'm up on the platform and it's my job, I find I'm pretty cool. I'm where I'm supposed to be.

I received a lot of compliments for the way I handled the New Orleans business meeting. I was very pleased the way it turned out.

##

Riess: Which was the meeting when the Native American groups were very vocally expressing disaffection with anthropologists?

Foster: I remember at the San Diego meeting a group of Native Americans came into the meeting and took over the platform for a few minutes and made some statements denouncing anthropology and anthropologists. Then they got up and filed out as a unit, and we went on with the meeting. That was the first time that had happened. [laughs] *Robert's Rules of Order* didn't tell us how to handle that!

Fellow, Center for Advanced Studies in the Behavioral Sciences

Riess: You said you were gone from Berkeley much of that time in the late sixties what with your research and the year at the Center for Advanced Study in Behavioral Sciences in Palo Alto. You moved to Palo Alto?

Foster: Yes, to Menlo Park.

Riess: That's a lot of time to be away. How did that work?

Foster: I was on sabbatical in Mexico for the entire academic year of 1964-65. Then I was in Mexico again, for six months, the winter and spring quarters of 1968. Following this I was on leave-without-pay at the Center for Advanced Study in the Behavioral Sciences at Stanford for the academic year 1969-70.

Riess: This was the decade of your major writing.

Foster: I did a lot of writing in that decade, yes. In the seventies, too. At the Center I felt in a way defrauded because the Student Mobilization Committee and the AAA ethics committee affair took up most of my time. I wrote one excellent paper, my "Anatomy of Envy." I brought that to fruition there and presented it at one of the regular meetings. I had a lot of help from other people.

Riess: One of the regular meetings down there?

Foster: Yes.

Riess: So that atmosphere was good?

Foster: It was a wonderful experience, yes, but a year marked by tragedy also. One lovely spring day I arrived by car, and as I started up the hill to the parking lot I was stopped by police. It turned out that the Center had been torched in the early morning, and half a dozen offices and their contents were destroyed. I was lucky, my office was unharmed. Sol Tax lost all of his notes, as did the anthropologist Srinivas, from India. He wrote a book called *The Forgotten Village* entirely from memory, since his notes were practically all destroyed.

Riess: Why the torching? Was this political?

Foster: Just political. I guess the scholars there were viewed by some of the Stanford students--it was assumed they were Stanford students, though they never were able to prove it--as fat cats, and they

didn't understand things and ought to be punished. It was just the tenor of the times.

Riess: Did people's past choices during the loyalty oath--did the loyalty oath as an issue come up again at the time of Vietnam?

Foster: The loyalty oath took place before I came back to Berkeley in 1953.

Riess: Yes. It had been so divisive. I wondered whether there were the same kinds of splits later.

Foster: I think the loyalty oath, as I understand it, was much more powerful in splitting the people than anything that followed afterward. I would guess it was. How many people resigned over the loyalty oath?

Riess: I don't know.

Foster: Twenty or thirty, a large number. I don't know anyone who resigned over the Student Mobilization Committee or anything else during the sixties and early seventies.

Riess: Did anthropology students help with voter registration in the South?

Foster: Richard Currier was the student who was most active. I don't know if he did that or not.

Currier subsequently did a wonderful dissertation on the Greek island of Ios. He submitted it, at my recommendation, for publication in the Cal Press, and the anthropological outside readers gave it rave reviews. Phil Lilienthal finally turned it down. I asked him why, and he said, "Well, I have a friend at Stanford who has a summer place on Ios, and he tells me the book is no damn good." I always held that against Phil, I thought it was shameful--I've never shared this with anyone until now.

Ernestine Friedl, who was a past president of the A.A.A. and who had worked in Greece for many years, gave it a rave review, as did the second reader, whose name I don't recall. To reject a manuscript because of the unfavorable opinion of an anonymous non-anthropologist, a friend of the editor, who had a summer home on the island--this seemed to me to be unprofessional behavior. I really have never understood Phil's reasoning. I liked Phil personally, and Mickie in subsequent years, as a member of the board of the Ploughshares Fund, has become very close to Phil's widow, Sally.

Rejection of the manuscript took the heart out of Currier. He left anthropology. Now he's doing very well as the president of a computer company, so he's worth much more financially than he would have been. But he was a splendid anthropologist--it's a great loss to anthropology.

Riess: He was also involved in civil rights?

Foster: He was a student whom I was close to. We knew each other's positions and respected each other and liked each other very much.

Riess: In those years there were three new university campuses: Irvine, San Diego, and Santa Cruz. Did they create anthropology departments automatically, and were you advisory?

Foster: Not in the creation of the departments. I had nothing to do with any of the schools.

The Vietnam War, Under Discussion

Riess: I wonder, did the divided view of the Vietnam war in this country play out in the Triple A in a way that might be explained by how anthropologists look at the world?

Foster: Well, I'd say that it reflected the difference in attitudes between the Second World War and Vietnam. In the Second World War, the anthropologists had their annual meeting just two or three weeks following Pearl Harbor, and the association formally offered all of its services and the services of all its members to the United States government. And of course, half of all anthropologists were engaged in direct wartime work--many of them in intelligence work in the OSS. That was the image that a lot of the older anthropologists, including me--I was not old at the time--but that was the image we had, that it was the duty of the anthropologist to aid the government.

Many of us were slow in recognizing that the Vietnam War was a very different type of thing. I was slow. I subscribed to the domino theory for a long time after I should have realized better. But I think it was the gap between those of us who lived through the Second World War and thought of the government as doing the right thing--as it was, I certainly think--and felt that it was the obligation of an anthropologist to be helpful, and the recognition of many of the younger people--and some of the older people as well--that Vietnam was a very different kind of a situation and we had no business being there.

Riess: What brought your thinking around? You say you were slow in recognizing it, but then you recognized it.

Foster: No specific event. I just gradually came to see that I had been unduly conditioned by my earlier years' experience, that this was different.

Riess: Did the Little Thinkers thrash out Vietnam and Free Speech and ethics? Could you bring these dilemmas to the group?

Foster: I'm sure we talked about it, and I'm sure there were all different spectrums represented, people from Stephen Pepper to Alexander Meiklejohn.

Riess: Was that where your mind got changed?

Foster: I'm sure that any university group that meets regularly has an impact on the thinking of all the members. I can't remember any specific thing. I remember disagreeing with a lot of things that were said, but I'm sure I was influenced by much of what was said, though I can't think of any specific examples.

Riess: During those times, the late sixties and the seventies, did anyone resign from the group?

Foster: Not at that time.

The only person I can think of who resigned from the group was Phillip Johnson, from the Law School, seven or eight years ago. Phil Johnson is an interesting person. He's very bright. He was a law clerk for Earl Warren. But he turned out to be strongly anti-evolutionary in his thinking. He's a strict creationist. His first book was called *Darwin on Trial*. Sherry [Sherwood] Washburn, a physical anthropologist, was a member of the group at that time. Somebody asked Sherry what he thought of the book, and Sherry steamed a little bit, and burst out, "That's the worst book I've ever read." Phil just laughed.

I was not there when something happened between--Joe Tussman said something that offended Phil Johnson, and Phil never came back after that. That's the only case I can remember of a member resigning over an issue.

Riess: Those people were very strong figures. What a forum!

Foster: Henry Nash Smith was another powerful character. David Krech. They're the ones that I particularly remember.

Riess: It was Tussman who started an experimental college?

Foster: Yes, he had Tussman Tech--a local Great Books project. He has written about his experiences, and it's very, very interesting. Joe has become far more conservative than he was at one time, as many of the members are. I was the token conservative when I joined; now I'm the token liberal in the group. I don't think I've changed as much as the constitution of the group has.

Riess: Is that a product of age?

Foster: It's a product of age. I think most people don't become more liberal as they grow older. And they may really not have changed, but the times change around them, so they appear to be more conservative than they once were. My mother was the exception. She became much more liberal as she grew older. I like to think that I followed after her.

Riess: Because of your training do you find yourself being a disengaged observer sometimes, more than other people?

Foster: I don't think so. I told you about asking the group how many of them considered themselves atheists, and how I was kind of surprised that they were reluctant to confess that they didn't believe in religion at all. [laughs] I've made the statement from time to time that I think organized religion has a great deal to answer for. I've asked the question, "I wonder what life would be like if man hadn't had the lack of wisdom to invent God in his own image," and that kind of shocks some of the people. I'm surprised that it does.

##

Riess: How did you take up the Vietnam debacle, or whatever you might call it, with the students in your classes. Did you address this, which was sort of out in the air, in your classes?

Foster: Not very much. I, perhaps mistakenly, have always made a point of not discussing religion or politics in class.

Riess: That would have been hard to do in those days. Didn't you have a few shrill voices in your class?

Foster: Yes, but I avoided it by meeting my class at eight o'clock in the morning. Revolutionary blood runs pretty sluggishly at eight in the morning. [laughter]

Riess: Am I to take that seriously?

Foster: Well, for a long time I gave my major class at nine in the morning. After it hit about two hundred, I thought that was too

many, so I changed it to eight o'clock, and that cut the attendance to a manageable one hundred. That was more or less chance. At the same time, the students began to ask questions and raise issues. At that time in the morning, though, they weren't very anxious to get involved. They were saving their energy for later in the day, I think.

Riess: How about in the Academic Senate?

Foster: The Academic Senate was always just a horror. Just to sit through the Academic Senate was the worst punishment I ever had. The claptrap that was raised is beyond belief--and I don't call it claptrap for nothing. I always think of David Krech, who would sit, boiling, about any topic that came up. Once every meeting you'd see his blood pressure rise, and his face would flush. Finally he'd get up and make an impassioned speech about something, whatever it was. Then he'd sit down and just beam like a baby the rest of the meeting.

There were people like that at the annual meetings of the American Anthropological Association. There are a few people that always do that. It's a pattern of behavior I find very interesting. They feel they've got to make a statement, and they work themselves up, and once they make it, the pressure is gone, and they sit down and relax and enjoy the rest of the meeting.

Riess: As a psychologist, could he see what was happening?

Foster: I don't think he did.

Riess: Are you talking about the sixties and seventies, or are you saying that the Academic Senate is painful at any time?

Foster: I'm thinking of the years following my tenure appointment, to perhaps 1975, years when I felt obligated to support the system. Everybody was supposed to go to the Senate meetings while I was active. Now they've cut it down. They've got committees, I guess, that handle it, so most people don't bother to go to the meetings at all.

Riess: Did you have committee appointments that were time-consuming?

Foster: No. My efforts went largely into the department. I was never a member of the budget committee or any of the major committees. I was a member of a good many ad hoc appointment committees and things like that, but I never was really a member of any of the major committees. I shunned them.

Lincoln Constance, who was a wonderful dean, sounded me out as a possible successor to him, but I discouraged this. I said I thought I could do more in the department, and I think I could.

Anthropology Department Dynamics

Riess: I want to pull a few questions out of *History at Berkeley: A Dialogue in Three Parts* by Gene Brucker, Henry May, and David Hollinger.⁴ One of the questions that the book brings up is the residual effect of the loyalty oath, which we just touched on. Was there any residual effect from the loyalty oath that you could see in the anthropology department?

Foster: Not at the beginning in '53, when I came back, I couldn't see any at all. The only people that were still active who were here at the time--and they had come recently--were John Rowe and David Mandelbaum. Lowie had retired then. Gifford was teaching. But the loyalty oath--I don't think had any effect on the anthropology department particularly.

Riess: The notion of the patron-client relationship--as they put it in the book--of senior and junior professors I thought was interesting.

Foster: I did, too. But I don't think that applied to our department. Kroeber must have retired about '48, when Mandelbaum came. I think that's what split Kroeber and his life-long friend, Sauer. They ended up as bitter enemies. Kroeber wanted to bring in Julian Steward, as I've heard the story told. Sauer was equally bitterly opposed to Steward. They settled on Mandelbaum.

And then John Rowe came a year or two later--John and I had worked together in the Smithsonian, the Institute of Social Anthropology. John is an ethnologist-archaeologist, historian particularly, a very excellent person. He is younger than I am by seven or eight years, I guess.

So at the time, there was really no older person to have a client--no older patrons. Kroeber and Lowie had always been very good. They had students, but they never had disciples. I think

⁴*Chapters in the History of the University of California, Number Seven*, Carroll Brentano, Sheldon Rothblatt, Editors, Center for Studies in Higher Education and Institute of Governmental Studies, University of California, Berkeley, 1998.

that's a very important distinction. Perhaps we've discussed this, but when I look at the kinds of activities that Kroeber and Lowie students went into, it's amazing. Duncan Strong was a major archaeologist. Julian Steward was one of the first ecological anthropologists. Bob Heizer was the best archaeologist, probably, that ever came out of this department, and an all-purpose anthropologist. Margaret Lantis, Arctic ethnography. Katherine Luomala, a top-notch folklorist. Cora Du Bois, one of the very first culture-and-personality people. Ralph Beals, the first Mexicanist.

People went into all different fields. They all had the same training, but they were free to pick their area of interest, and they did. So we had no tradition of patron-clients. Nobody was expected to follow the masters. We all decided on our field of research and did what we wanted to do, so we escaped that. By the mid-fifties, the only people in the department were Edward Gifford--he retired in '55, which made possible my permanent appointment--and Ted McCown and John Rowe, and David Mandelbaum, and that was it. I was the next person to come in.

So we really had no old-timers that were trying to force students to do particular kinds of research. I know you can look at the blow-up with Schneider and Geertz and Fallers in those terms, but I don't believe that's true. At the time, Ted McCown was the oldest person in the department--he must have been fifty-three or four. Dave Mandelbaum was fifty. I was forty-eight, I think. So we were hardly old-timers.

And no one was trying to force Schneider to teach certain courses. He was free to teach anything he wanted. What some of us objected to was his attempt to recast the whole department in his image. We thought--and I still think--that a department is better and more stable if it has some tradition of continuity. Some respect for a fine department, I think it should recognize its ancestry and at the same time be open to new ideas. I think we were.

Nobody criticized or told Fallers or Geertz or Schneider what they had to teach. They were free to teach anything they wanted, to form associations with anyone on the campus. As I say, they worked closely with Erving Goffman, who was very influential in anthropology and sociology at that time.

Riess: I understood patron-client to mean a powerful older person in the department trying to ally some of the younger faculty with him.

Foster: Yes.

Riess: But you said to me that your choice in recruiting was to recruit well-established people when you could afford it, rather than bringing in young and malleable people.

Foster: I think that's why we became a top-notch department very rapidly. We were able to make a number of full professorship appointments of people who had been around long enough to demonstrate that they were not just flashes in the pan but they were still young enough to have a lot of fresh ideas, and for the foreseeable future-- people like Washburn and [Elizabeth] Colson and Desmond Clark and a number like that.

Riess: So they came in as peers.

Foster: They came as peers, not as beginners.

Money for Everything, A Glorious Period

Riess: Another thing they mention in the history of the history department is support from Clark Kerr and Lincoln Constance. You also referred to that. That means supportive in recruiting?

Foster: Yes. They treated our department very well. We made good cases, I think, for the people we wanted. That was a glorious period. There was money for everything.

Riess: Yes. You brought in so much of that money.

Foster: We have talked some about this. But I have often thought that was my most important contribution to the department. I brought in \$3 million, approximately, in three different units over a period of fifteen years, all of which went to support graduate students. The only part that didn't support graduate students was one secretary. We had over a hundred graduate students on grants. It was marvelous. That was from '64 to '79.

That's the way we got started in medical anthropology. I had had the good luck--chance has governed my life in every way-- I've told how I turned to medical anthropology or international health when I was at the Smithsonian, to get money to keep the program running. That brought me into contact with a lot of the leaders of public health. C.E.O. Winslow, who was one of the greats at the Yale School of Public Health; Hal [Wilton L.] Halverson, who was head of the California State Department of Public Health; and Ed McGovern, who was head of the North Carolina School of Public Health.

I knew a great many of the people in Washington in public health, as a result of the little bit of work we did. And fortunately, that reputation stuck to me for a few years, so that when this money became available, I was in a good position to know the kind of application to write. Obviously we got the money, and we delivered the goods because we turned out a great many fine anthropologists during these years.

That's what led to our decision to create the medical anthropology subspecialty with the University of California at San Francisco. I think that's one of the--it's the only anthropology joint degree I know in the university system, and it may be one of the very few Ph.D. degrees given jointly by two branches of the university. Students register either in San Francisco or Berkeley, and they do the bulk of their course work and their records are kept in one or the other, but the degree is given by the University of California, UCSF and UCB. It's neither a Berkeley nor a San Francisco degree, it's a joint degree. That's worked very well. It's had ups and downs, of course, depending on who the personnel are.

Riess: But it's a unique program still.

Foster: I think it is, yes.

Riess: And people go on to what kinds of positions?

Foster: Well, they go on to teaching positions, government jobs. They've done very well. They're leaders in the field.

Riess: To go back to this time of a lot of money, it seems to me, first of all, that if you have well-supported graduate students, of course you're going to get the cream of the students. And then, knowing that you've got the best graduate students, then it's kind of easy to get the best faculty.

Foster: Yes. Those were the glory days.

Riess: How did you communicate with Kerr? Was he easy to get to?

Foster: I didn't deal with him very much. I dealt largely with Lincoln Constance and, if necessary, with Glenn [T.] Seaborg, who was chancellor at that time. Clark had gone on to become president, so he was not immediately concerned with what was going on in anthropology.

Fostering Departmental Collegiality

Riess: In the history of the history department they give examples of fostering collegiality in the department and esprit de corps and a common vocabulary. Can you relate to that for anthropology?

Foster: For about six or seven years, 1959-65, I'd say, the social anthropologists used to meet once a month in somebody's home or sometimes at the Faculty Club for dinner. Somebody would read a paper. That was very, very, very good.

Riess: And everyone would come.

Foster: This was mostly the social anthropologists, but the others could come if they wanted to.

During this same period, or a bit earlier, the department participated in a very interesting inter-university series of meetings. This was a time when it was still possible during rush hour to get to San Francisco, or to Stanford. We had three meetings a year. Berkeley would sponsor one; Stanford, the second; and San Francisco State, the third. Somebody would read a paper at that meeting, and that was very good because it brought the faculty of the three departments together. That was nice.

Riess: How did that get started?

Foster: I don't remember how we got started on that. Bob Murphy in our department did a fine job in organizing and overseeing these meetings. We all enjoyed seeing colleagues from the other major bay area campuses, but it got to the point where traffic was so bad that the idea of driving to Palo Alto or even San Francisco during rush hour became unattractive. In addition, ad hoc series, things like that, tend to run their courses and attendance begins to drop off.

Riess: What do you mean "things like that"?

Foster: The monthly social anthropology meetings, and the three-times-a-year inter-university get-togethers--with the passage of time and changes in personnel, more and more people lost interest because of conflicting meetings or simply the press of work, and the groups dropped in size to the point of no return.

Shortly after I returned to Berkeley in 1953, and before my permanent appointment in 1955, a Social Science Colloquium was organized, and I was asked to take part in it. Milton Chernin was one of the active members of that. We'd meet in somebody's home

and discuss various topics. That was the broadest-based group in which I was actively engaged. It was a stimulating experience, but after several years it ran out of steam and quietly expired.

Jews, Women, Minorities

Riess: The question of Jews on the faculty turned out to be important in the history department. Was being Jewish ever a problem in anthropology?

Foster: The question of being a Jew has never been raised, to my knowledge. Lowie, of course, was Jewish. He joined the faculty in 1917. Because Boas was Jewish, a good many of the major anthropologists were Jews in the country. Herskovits, my first professor at Northwestern, was Jewish. It was just never even raised, the matter of race. I was amazed once when I was going East--I had known Gene Hammel as a student and a colleague for many years--and he said, "My father is a cantor." It never occurred to me that he was Jewish. And Alan Dundes, it never occurred to me he was Jewish. I didn't know he was Jewish until several years after he came here.

In anthropology departments I have never known race or ethnicity to be a factor in making or not making appointments. We have had our disagreements in the department, but they have never centered on race or ethnicity.

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Riess: And minorities in recruiting?

Foster: The first--women were considered minorities for this purpose. Cora Du Bois was asked to join the faculty during the loyalty oath period. But because of the oath, she turned it down. She would have been the first regular female appointment. Laura Nader turned out to be the first one. She was hired not because she was a woman but because she was, we thought, the best person available. Nancy Gonzalez, Nancy Solien Gonzales we had as a visitor for a year, but she knew we were not sizing her up for a possible ladder appointment, that we had invited her for a year to replace someone on leave.

And I told you how we found Laura Nader.

Riess: Yes, and I was amazed that you could tell so quickly.

Foster: I guess I just was struck by her strong personality. She loved the field (field research), as I do. When she described what she went through in Mexico to get to the field, I thought, "This is a real tough woman." I've often thought Laura can take satisfaction in her appointment in that she was not the best woman available, she was the best anthropologist available. We could have hired a man just as easily. There was no question of anything like today's equal opportunity employer statements.

I remember talking with her, and I asked questions like, "What do you plan to do in the future?" and she laid out her plans for research for the future. I can't remember at all what she told me. I asked all of them pretty much the same kind of questions. And I can't even remember who the others were. But she just seemed to me to be the appropriate person.

I've often thought how easy the chair's job was in those days: no need to advertise positions, to think of "equal opportunity" with respect to non-discrimination, and the like. The present system, for all of its cumbersome and time-consuming aspects, is obviously superior, in that it forces us to look at a much broader spectrum of candidates, resulting in a better ethnic and gender balance than the old system produced. And it is clearly far more fair to women and ethnic minorities.

Our first minority--black--must have been John Ogbu, I guess, who was one of our own people. There were very few black anthropologists, of course, thirty or forty years ago.

Riess: Was he African?

Foster: He's Nigerian, yes. He, of course, went into education, educational anthropology. He's one of the top people in the country now. He had an immense offer from Columbia. He's had offers from North Carolina and other schools. But he decided to stay with Berkeley, fortunately.

And William Shack has also been a fortunate choice for us, an African-American social anthropologist who has done outstanding research in Ethiopia. His personal and professional qualities have been recognized on campus by service as Graduate Dean, and as president of the Faculty Club.

Riess: What is educational anthropology?

Foster: The application of anthropological concepts to educational policies and programs. I can't give you a very good idea. I've not been involved in it myself. But it's a major area.

Riess: And Hispanics?

- Foster: We had no Hispanics at all while I was chair of the department. Since then we've had an occasional visitor, but to this day I don't believe we've had a tenured Hispanic staff member. We have also had fewer Hispanic graduate students than I would have expected. I had a very able student from Peru, Jorge Osterling, who took his Ph.D. with me. But oddly enough I have never had a graduate student from Mexico. We are now getting some very promising Mexican-Americans, for example, Robert Gonzalez who took his degree last June with Laura Nader and is teaching this year in the department as a visitor.
- Riess: In fact, in a way ethnic studies could have been a department of anthropology, couldn't it?
- Foster: Could have been, yes.
- Riess: Is there a story there?
- Foster: None that I know of. I think it's just as well for anthropology that it [ethnic studies] is not a part of anthropology. I think it would become too political. The very nature of ethnic studies means politics, and I'm glad the department is not any more political than it is.
- Riess: And some course offerings in the history department look like they could be offered in anthropology.
- Foster: Yes, yes.
- Riess: What happens? How does that get thrashed out?
- Foster: There's the course committee that has to pass on all new courses that are offered, but that's, I think, pretty much routine. Every department has the authority essentially to decide what it wants to teach.
- Riess: So if the history department is teaching something called "Culture and Society" and anthropology is teaching "Society and Culture" or whatever--.
- Foster: I think there would be no problem. The assumption is the point of view would be so different--one historical and one cultural, anthropological--there would be no reason not to have the courses. For a long time, I thought that medical anthropology and medical sociology would be the same thing, but Virginia Olesen, who was a medical sociologist at UCSF, and I had a series of meetings, and we wrote a pair of articles on this. We concluded that both of them were valid, that they were very different in content and point of view. Although we started out with the assumption that we would find we were teaching the same thing under different

titles, we found they were very different, both in methodology and basic concepts.

I'm a stronger believer in the fact that the traditional departments have more of a basic core, an honest core, than many people think. A great many people feel that all knowledge is something that by accident was chopped up into these units. To some extent that's true, but I think there's a good reason for most of the departments. I have never been keen on joint majors for students. I think they should learn the basics in one field, and then they're free to do anything they want.

Riess: That leads into the next question. Talking about the seventies as [reading] "a time of loss of clarity as to what anthropology was...a new heterogeneity, a vast range of interests, 'by becoming everything, it threatens to become nothing.'"⁵

Foster: I think that more applies to my post-retirement years. I suppose that refers to post-modernism and all that, which I have not followed very carefully. I've not been sympathetic to it at all.

Riess: It's a hard thing to follow.

Foster: It is. In the most recent, current *Anthropology* there's a long discussion about the validity of the use of the term "culture." The idea of throwing overboard the concept of culture just shakes me, rattles my back teeth. I just can't imagine having anthropology without having the concept of culture.

Riess: Thinking of what you said about David Krech--are you going to turn red and come to a boil and write a paper about it?

Foster: No. I don't do that very much. I've often said in recent years I don't know whether I would become an anthropologist if I were starting over today. I'm just not at all sympathetic to the examination of one's navel, which seems to me to be much of contemporary anthropology: theory without data. My interest in anthropology has been mastering a body of data and noting the curious quirks that don't fit the pattern, and then trying to figure out answers.

I've often said that my theory basically emerges from my field research. I feel that data without theory are useless--perhaps not useless, but of secondary value. But I just can't see how one can concentrate largely on theory without having a substantial body of data under one's belt. It seems to me that so

⁵See discussion in Interview #9.

much contemporary writing is just theory, and poor theory, a lot of it.

And the contemporary style of writing--by the end of the first two or three paragraphs my interest in what I suspect is to follow has been killed. Almost all anthropologists today feel they must cite everyone who has ever had anything to do with their topic. Not infrequently I find myself cited by the entire book without the slightest indication as to why; I wonder, Why am I here? I have no business being here in this list of citations. The potential life in so many articles has been killed by being interrupted continually by meaningless citations. The reader never can decide what the meat is.

I like the old-fashioned system, under which I was raised, which I still think is the best. When you put something in your bibliography it's because you cite it, and you include pages, too, so that readers could go and check if they wanted to. The idea of just throwing in a whole series of books vaguely related to the theme is sheer literary gorgeousness.

Riess: I like that expression.

Foster: It's Mark Twain, speaking of the German language, and it has always amused me.⁶

An Argument for More Attention to the History of Ideas

Riess: There are a lot of biographies of anthropologists and literature on the intellectual history of anthropology. Is this a good sign?

Foster: I think it's very good, yes. I think we haven't paid much attention to our--well, we've paid attention, that's not a correct statement. But what interests me is how little attention anthropologists have paid to how our basic concepts have developed. For example, we know how the culture area concept of Clark Wissler developed because he tells us about it. But how did Redfield's folk-urban continuum concept, which was a major theme for twenty years or more--how did he get the idea? We've got very little telling about how we've come to the conclusions we've come to. I'd say that's one of the things that interests me most.

⁶Found in Appendix D, "The Awful German Language," in *A Tramp Abroad*, by Mark Twain, 1880.

Did I by any chance loan you--I hope I did--the *Long-Range Studies in Social Anthropology* book?

Riess: Yes.

Foster: You've got it, good, because I was looking for it, and I couldn't find it, and I was worried.

In that book, and in the manuscript of "A Half Century of Field Research," that's one of the things I discuss. I think it's very interesting. My article on "Peasant Society and the Image of Limited Good" is by all odds the best-known thing I've ever written. It has been widely cited in past years. I tell in the book about how I got the idea, and I discuss several other major articles, telling how the ideas came to me. I wish--if I were younger and organizing a workshop, what I would do is get together a group of anthropologists who have been theoretically oriented to explore the theme. I think it would be fascinating.

Riess: It seems like an obvious part of any anthropologist's narrative, that they would start with where the idea came from. That's interesting that it *doesn't* happen.

Foster: I don't know why it is. I think it's not that I just don't know the literature. There may be a bit more attention paid to it now than I think.

Riess: Is there anything else that reading the history of the history department brought to your mind?

Foster: I wondered if it was simply an extreme case of something that all universities faced during the same period with the vast increase in enrollments--not the lack of elitism in universities, but the opening of the doors to people who would never have gone to universities, the vast increase in numbers, and faculty members who were not from the intellectual or economic elite, as many of them were before. I wondered if that wasn't a special case, by local circumstances, of a general process. I don't know, I've never really thought about that.

But I suspect that any department worth its salt underwent some kind of a shift during the fifties as a result of the government support of veterans in universities and the vast increase in numbers. When I was an undergraduate, I think there were half a million students at all levels in higher education units. Now the number must be what? Close to twenty million, I guess. When I came to Berkeley, there were eight thousand undergraduates and one thousand graduate students. And that was a

huge place on the West Coast, must have been the biggest university by far on the West Coast.

So any sudden increase in size in institutions is obviously going to have a major impact on the structure and the nature of the content.

Riess: And the word "elite" has to drop out of the vocabulary.

Foster: Yes, it should. I think there were no black professors at all--except in black universities--until after the war. Very few, at least. David Blackwell in statistics, he was the exception. I knew no others at Cal. And very few women. Emily Huntington in economics, she was the rare exception.

Riess: And Josephine Miles, a woman, and disabled.

Foster: Josephine Miles. My, what a pleasant person she was.

Riess: Do you think a book like this history of the history department will be written about the anthropology department?

Foster: I don't know anybody who's doing it, I doubt it. It's very worthwhile. I wish we did have one like that, but I don't know how we could get any kind of agreement.

Riess: Well, they had three authors.

Foster: That's the way it would have to be done.

Bygones, and Shaking Hands with Eric Wolf

Foster: About 1970, give or take a few years, Gerry Berreman recommended that we hire temporarily a sociologist named Ernest Becker, who was a charismatic character. Berreman explained to us that Becker had been in the UC Sociology Department. They didn't have a place for him: they wanted to keep him, but they couldn't, and that he had a year before he was going someplace else. So it would be just for a year. So on those terms we hired him.

He was charismatic and had a vast student following. Toward the end of the year, it turned that out he wasn't going anyplace, and Berreman pushed heavily for him to be a regular member of the department. There was a great deal of student support, but most of us thought that Becker was not the person--I forget whether he taught another year or not. But that caused a lot of ill feeling

in the department. First of all because we felt we had been sold a bill of goods, and secondly because it just caused unnecessary strife.

Later I talked with Herb Blumer, who was chair of sociology at the time, and he said that they let Becker go because they didn't think he had anything, that he was floss and fluff, and that what Berreman told us was totally false. This was a shock. But Becker did leave. Well, it caused a lot of comment from the students, who were angry at us. Becker died a year or two later of something. But that was one of the unpleasant things in the department. It might have been '73, when I was chairman for a year.

I'd like to say on both Berreman and Wolf, I have been very angry at them at times, but I find that when the circumstances causing strife are passed, I tend to forget the past. Berreman is now a good friend. For about ten years, I just couldn't bring myself to talk with Eric Wolf, but about fifteen years ago, about 1985, I was at an annual meeting, and we were at the big reception, and I walked over and I stuck out my hand and said, "Eric, I think it's time we shook hands and were friends again." He was relieved, too. I've not been close to him since then, but we're on good terms, and I am relieved at that.⁷ I don't like to think of going to my grave angry at people.

Riess: Yes.

Foster: I've never been able to understand his position. It just seems to me that it violates all the tenets. A person is innocent until proven guilty.

Riess: So you haven't had the one more conversation that you might have.

Foster: We'll never have it. It would be pointless to try to thrash that out with him. He feels just as passionately that he was right as I feel that I was right, I'm sure.

Riess: Why was it taken up by the *New York Review of Books*?

Foster: Well, the *New York Review of Books* is a very liberal journal.
[pause] I don't know. I know I wrote--my letter was in their

⁷At the annual meeting of the AAA in Philadelphia in early December 1998, I had a good talk with Eric--about generalities, and not our celebrated feud. He was dying of cancer at the time, and I knew I never would see him again. I'm glad we had this last opportunity to express our mutual respect and friendship. [Eric Wolf died March 7, 1999.]

hands for six months. They didn't even acknowledge it, I didn't even know if it was ever going to come out. I held that against them. I think they should have responded, "We have your letter. We'll publish it in due time." I had given up hope that it was ever going to come out, and I thought it was very unprofessional of them.

Riess: This airing of the dirty linen of anthropology--was it a good or a bad thing, do you think, in the larger view?

Foster: Oh, I don't know. A ripple, probably, in the intellectual life of the country--a minor ripple.

Thoughts on the Serendipitous in Discoveries

Riess: Another subject. You have said how important Thomas Kuhn's thinking was for you.

Foster: It was his idea of paradigmatic models that I found interesting.

Riess: Did you know him when he was here on campus [1956-1964]?

Foster: I didn't even know he was here. I picked up the book and read it. The book was published after he left Berkeley, I think it was published in '62 or thereabouts.⁸ That's when I first became aware of him. I didn't know him here at all.

[reading] "Thomas Kuhn's paradigmatic model, which stresses the importance of anomaly in scientific breakthroughs is relevant here. 'Discovery,' he writes, 'commences with the awareness of an anomaly, that is, with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science.' In my case, this awareness has been triggered by serendipitous, that is lucky, unplanned observations." And so forth and so on. (That's on page 21 of the unpublished "A Half Century of Field Research in Tzintzuntzan.")

In trying to figure out--you asked me to work on the question of what unifying themes there may be in my career, because it looks as if I jumped around a great deal, and I have. But I'd say it's methodology, the way I approach data and theory--

⁸ Thomas S. Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press, 1962.

gathering a lot of data on a particular topic, and being alert to the odd bit that doesn't fit the pattern, and then asking myself how can I account for this. I think I've been pretty lucky in working out some ingenious solutions. You can't prove things in social anthropology the way you can in physics and chemistry. You can just say that this is an economical explanation that seems to fit the facts and it seems better than explanation Y or Z. I'd say methodologically, that's been a unifying theme in my work.

The other is a long-term interest in processes of social change. That's spelled out, of course, in the "Half Century of Field Research" manuscript. And my first real book, as against monographs, *Traditional Cultures and the Impact of Technological Change*, is a statement at the time of how I viewed--what were the factors that made for change and prevented change. I've always been interested in that.

Riess: Do you think back to Ottumwa--when you think about long-term change, I wonder whether you've looked at your own village.

Foster: I used to go home until my mother died in 1982, over a period of years. Yes, I thought of it a lot, a great deal. Anybody who reaches the age of eighty-five can't help but be astonished at the changes that have come about in our lifetimes. So different.

Riess: Do you find yourself forming theories there? Or does your mind work differently in Iowa or South Dakota, or Berkeley, than it might work in Mexico?

Foster: Mickie tells me it works differently. She says I'm totally uninterested in my own society. I don't think that's true, but it's true I have not written much about my own society. I've been concerned about medical bureaucracy in our society, which is certainly our society. But I have not taken my life observations in Ottumwa, Iowa, and tried to weave them into a story.

Society for Medical Anthropology

[Interview 8: February 17, 1999] ##

Riess: Last time we sidled into a discussion of your presidency of the Triple A, and the experience with the ethics committee. I wonder if you've thought about that in the week since. Often when something like that comes up, it's in your mind.

Foster: No, I really haven't. I've been so busy, I haven't really thought about it.

Riess: You were on the Triple A executive board from 1957 to 1960. Would you say that there were warnings of problems ahead at all? Or was it serene?

Foster: Serene as could be. No problems at all. The association had been reorganized about 1948. Julian Steward had been one of the leaders and Father John Cooper at Catholic University. And I think Wendell Bennett at Yale. It was a very simple organization, a very small organization: six board members, a president, a president-elect--all elected, the eight of them--and a secretary, who was appointed, and an editor, who was appointed. That was the whole structure. It was a dream to run the organization in those days.

Riess: You were on the executive board of the Society for Medical Anthropology, following your position as the head of the Triple A. Was there any similar uproar in medical anthropology?

Foster: No. We were just getting started in medical anthropology at the time. I was not awfully active, although I was a board member. My principal contribution was advising the board not to put the dues in the constitution. Because the Triple A had had the dues specified in the constitution, that required a vote to raise it from six dollars to eight dollars, I think. So I urged them to put the statement of the dues in the bylaws so the board could determine the dues.

Riess: And the question of ethical standards? Was it appropriate to think of that in medical anthropology?

Foster: It was very much to the point, but the Society for Applied Anthropology was the first anthropology organization by all odds that devoted serious attention to the problem. Its first statement came out in 1949, I think, and it has been updated a number of times since. Most of the concern was for the people being studied, in all statements of ethics.

I was personally probably slow in recognizing the need for an ethics committee.

Riess: I'm surprised that you say that. You have a chapter in your book on long-term studies on the ethics of planned change.⁹ What is that?

Foster: Going in and sizing up problems of people, and trying to help them decide how to overcome the problems. That's quite different from just going into a village and studying it and then writing a monograph.

Taking Responsibility for Anthropology Ph.D. Curricula

Riess: I noticed in your bibliography a Wenner-Gren Triple A conference on Ph.D. curricula, which you organized. Why was that imperative?

Foster: That took place after the annual meeting of the AAA in Washington held the week before Thanksgiving, 1958.

The background of that is that I served as graduate student advisor for the second and third academic years following regularization of my status in July 1955, i.e., 1956-58. This was the first time the department had assigned this responsibility to a staff member. Prior to this time the chairman talked with graduate students discussing their course work for each semester. But since no formal record was kept, it was pretty haphazard; with no way of keeping track of students, they kind of drifted along. So I did a couple of things.

One was to draw up what I called a control sheet, a standard form on which we kept a record of every graduate student's basic data: age, undergraduate major and university, courses taken each semester, and the like, with space to indicate what the three fields were the student was to specialize in, who the advisors were, foreign languages the student was going to be responsible for, when they were passed, who had passed them. This system was a vast improvement over the old way of passing students along without a clear idea as to their progress. And at the end of each semester we had an all-day meeting at which every graduate student's progress was reviewed.

When I was a graduate student there was nothing at all like that. Kroeber just--after we had been around a few years--said, "Well, I think you better take your exams in a month or so," and

⁹ Long-term Field Research in Social Anthropology, edited by George M. Foster, Academic Press, 1979.

we would. That was all right when there were a dozen graduate students, but when we had fifty or seventy-five, that was not nearly enough attention.

Riess: Were the control sheets worked out with the students, so it was like a contract?

Foster: No, it was not a contract, just kind of an office memorandum, in case the professor who was guiding the student was on leave, the substitute had some knowledge about the student and what the student had done. Even for the professor who was responsible for the student all the way through, it was difficult to remember what each student had done without that. It was a very useful thing. I think they use it still. It has been revised, I'm sure, many times.

The other thing was to organize this conference in Washington that you mentioned. Although by the time the meeting took place I was halfway through my first year as departmental chair, I organized the meeting during the final six months of my time as graduate advisor. While serving as graduate advisor I also came to realize that we didn't know what other Ph.D. degree-granting departments expected of their students, and I suspected the other departments didn't know what we and comparable departments expected of ours. So at the AAA executive board meeting in December 1957 (I was a member of the board at that time) I proposed that a small committee be set up "to explore the possibility of a conference to discuss problems of Ph.D. curricula and training."

The board accepted the proposal, and named Emil Haury (University of Arizona) and Sol Tax (University of Chicago) and me as chair. So, to have a conference on the cheap, I simply sent around a letter to the approximately forty departments believed to be offering the doctorate inviting them to send representatives to a two-day conference that would follow immediately upon the annual meeting, so there would be no extra traveling expenses; and to circulate to all participating departments before then a description of their graduate program--the languages they required, the examinations, and things like that.

More than thirty expressed interest and, in point of fact, representatives from more than twenty-five institutions participated. We met for two days, and it was very, very interesting--a lot of variation. I think that was the first time that attention was paid to the form and content of graduate training, so we had an idea of what our colleagues in other schools were requiring. I've got a report on that somewhere. I'll look it up.

Riess: Were there some aberrant departments, or did that turn out not to be the case?

Foster: No, we weren't looking for aberrant departments. They were all doing a good job, doing an excellent job.

Riess: Did it lead to a kind of uniformity, or did it not have that effect?

Foster: It really had no follow-up. I don't know. I don't think it led to any more uniformity than we had beforehand. I remember everyone was very complimentary and thought it was very useful.

Riess: The control sheets you describe must be what is referred to in the Kroeber Anthropological Society papers when it says that you [reading] "developed the system of phased steps in the graduate programs, substituting meaningful scrutiny for formal hurdles."

Foster: I think so, yes.

Riess: And did you do away with some formal hurdles?

Foster: Well, we modified them. About that time, all graduate students had to pass a reading examination in both French and German. By then it was clear that that was not practical, that that was a function of an earlier age, when there was a lot of scientific literature in those languages, and when field work was not a part of physics or chemistry or anything like that, which was the basis on which it was drawn up.

It was a lot of work to get our graduate division to accept the fact that anthropologists needed a speaking knowledge of the language of the people they were going to study. It took us a year or so to get it through, but we finally did. That's been the basis ever since. We assume that the student will learn other languages, to read at least, if necessary. But we assume that a speaking knowledge of the indigenous language, or any language, in fact, is much more valuable than a dictionary reading language of French and German.

Now so much of the work is published in English. Even scholars in other countries, if they really want to be read, tend to publish in English. The French and German examination was a function of an earlier time in graduate training.

Riess: And it's never been okay to have an interpreter when you're doing your field work.

Foster: Not for serious work, no. There are situations where you do use an interpreter, but we assume that a graduate student doing a year's research--whether it's in Greece or Spain or New Guinea--will learn the language well enough so that he or she can communicate largely in that language. They'll certainly use interpreters for some of the more difficult things, and a research assistant may be very helpful. But ideally the anthropologist speaks the language of the people that he's studying.

In the case of my work in Tzintzuntzan, I'm far from a fluent speaker in Spanish, but I understand it well enough so that I pick up all kinds of cues that I'd never think to ask about.

Riess: You understand it well enough that some of your work has to do with the language.

Foster: Yes. I do all my interviewing in Spanish, of course, and I record my notes occasionally in Spanish but often in English. If the language is critical, I record it in Spanish. I'm a terrible linguist. In high school, I learned French to read.

My first year at Cal I had a German tutor. I studied three mornings a week with him, and I took the German examination from Lowie three times before I passed it. Then I probably forgot the language. I relearned it when I was writing the book on Spain, because a great deal of the critical, historical ethnographic data on Spain was in German. I've got a lot of German citations in that work, and they're legitimate, they're not just window dressing. I found them to be very useful. After that I forgot German again and haven't used it since.

More on Tape-Recording and Technology

Riess: When you say "record," you mean write? Or did you tape record?

Foster: I objected to tape recorders during a large part of my active life. I think we talked some about this earlier [see p. 72]. But I subsequently partially changed my mind. I concluded there are kinds of things where it's basic to have a tape recording. If you're recording dreams, for example, you want the exact words. If you're recording thematic aptitude tests, it's very important to have not only the words, but also the gaps, the things that are unspoken, which you never get.

When I was recording data on ethnomedicine, hot and cold, going over the tapes, I found a number of words that I didn't recognize. I thought I was getting it all, but I found expressions that would be used several times that I would never have been able to follow up if I hadn't picked it up there.

So I think the tape recorder certainly has its role, but I think it's a mistake for a student to start out and tape everything because 90 percent of what you're recording is going to be chaff. It's easier to winnow out much of what you're getting at the time you're getting it, even though you lose a bit of the grain. You assume you'll pick that up later.

As far as the technology goes, I told you about the Midge Tape. It was not altogether satisfactory. Ten years later I was using a regular Sony recorder that takes big flashlight batteries. They're very good. I still have a couple--one down in Mexico, and one here--but I haven't used them for a long time.

It's like video. I took movies in 1940, at the beginning of my very first field work in Mexico. And they're interesting, they're useful, but compared to what you can get from the camcorders of today, there's just no comparison. I had an Eastman magazine camera that took a cartridge of fifty feet of film that ran four minutes--it was all mechanical, I wound the spring, and it would run for four minutes. At the end of four minutes, whether I was in the midst of the most interesting thing I had seen or not, I had to stop, take out the cartridge, put in a new one, and rewind it. I'd lose a minute or two doing that. And then the film had to be spliced and edited, it was a real job. So for a long time, after my initial work, I didn't use movie cameras at all. It was just too much work to take stills and also record notes.

There's a marvelous new Sony camcorder that I'd like to get. My daughter, Melissa, had one in Mexico last Christmas. It has a cartridge that's about the size of the cartridge in an answering machine on the telephone, and yet you can blow it up and use it--you can have it converted to a full-size tape. It's a wonderful

gadget. And the audio at the same time, so you can tell what you're seeing, getting the sounds--I wish we had had that when I was active.

Riess: And you want to get one?

Foster: Yes. I hope to be in Tzintzuntzan for Corpus Christi this spring. That's the third of June. It's a wonderful fiesta. I've got a good deal of Corpus Christi on my 16 millimeter film, which is now in video, of course, but I'd like to get it on this and describe what's going on.

Riess: I wonder if the old language recordings have been converted into new formats. Is that something the library or the museum does?

Foster: I don't really know. The old wax cylinders that were used, I don't know whether they've been converted or not. That would be expensive, time-consuming.

There has been considerable change in keeping records in the field. Kroeber and Lowie took all their field notes in pencil or pen in a stenographer's notebook. I started out that way but graduated to a portable typewriter. Today, students use video cameras and recorders and computers, laptops. The technologies are marvelous that they have today.

National Geographic Explorers, and Amateurs

Riess: Recording a fiesta makes me think about *National Geographic*. Anthropologists must be very much part of the *National Geographic*.

Foster: Bob Heizer, the archaeologist, had a good deal of money from *National Geographic*, along with another Berkeley graduate named Phil Drucker, and Matt Stirling, who was an undergraduate major at Berkeley. He never took a Ph.D., but he became the chief of the Bureau of American Ethnology--he is the man who discovered the big Olmec heads--he didn't discover them, but he did the major early excavating in Veracruz.

The *National Geographic* has had a number of anthropologists as authors of articles, but fewer than you'd think, than I would expect--at least I don't recognize their names as anthropologists.

Riess: They ask different questions. *National Geographic*, up to a certain point, is really a picture book.

Foster: Yes. It's not really serious science, though it's more so than it once was.

When I was at the Smithsonian, occasionally I'd get calls from the *National Geographic*, asking for information about customs, and I would occasionally be asked to read an article and make comments. Smithsonian personnel were expected to do that on their own. Alexander Wetmore, secretary of the Smithsonian, was one of the *National Geographic* directors. So when the requests came, the idea never occurred to us to say, "Sorry, I'm busy." But it was interesting.

Riess: Earlier there were the days of the great explorers. I'm looking at a book about [Sir Ernest Henry] Shackleton.

Foster: Shackleton is my idea of an explorer. He had it all over [Robert Falcon] Scott and [Robert Edwin] Peary, he was so much superior. Although he was very much an amateur, too, he did all kinds of foolish things. But he was smart enough to turn back ninety miles from the South Pole. Peary, I think, was an out and out--not a fraud, he was a good man, but he was so anxious to reach the North Pole that he fudged his notes. I don't think he ever got there.

Riess: Those were great days, though, weren't they?

Foster: They were, indeed.

Riess: I guess there are a lot of amateur archaeologists. At sites in the Southwest, often part of the story is earlier amateur digs.

Foster: There have been grave robbers ever since there's been a market for anything one could sell. There's not much grave robbing among ethnologists. I use that word in a symbolic sense. I mean, in archaeology, there are only so many sites and so many artifacts.

In social anthropology, cultural anthropology, it's more a matter of interpretation. There's an unlimited amount of data. The fact that some anthropologist gets a body of data doesn't mean there's any less when the next one comes along, though there are those who do look upon it as a limited good.

Riess: There must be places where a bumbling anthropologist has muddied the waters.

Foster: I'd say largely by irritating the local people so much that when the next anthropologist comes along, they're very suspicious of him. It's certainly nothing like archaeology, where you can ruin a site, or do great damage, or where you worry about a road going through and tearing it up in a matter of several days.

IX CONSULTANT ABROAD

A Network in Applied Anthropology

Riess: Let's go to the main topic for today's interview, consultancies and seminars and workshops, and how that all began. Last week you talked about the mimeographed paper, *A Cross-Cultural Analysis of a Technical Aid Program* [1951].

Foster: That was the beginning of it all, and we might review the background.

I had been brought up, anthropologically, so to speak, to have no truck with applied anthropology. Neither Kroeber nor Lowie nor Herskovits would give it a nickel's worth of time. I followed them slavishly. But there's nothing like the threat of losing your income to make you change your fundamental assumptions, I've found. [laughs] And I realized that if the personnel of the Institute of Social Anthropology were to keep on getting an income, we were going to have to invent something new.

I knew about the Institute of Inter-American Affairs, having spent four months working for them back in 1943.

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Riess: And the paper?

Foster: The first work that was done by the people whose notes I edited for this mimeographed article, caused a real sensation, I must say, because we found answers to so many questions that were puzzling public health personnel, who were, many of them, having their first international experience. They expected everyone to come running to embrace them and their ideas. They were disappointed and hurt because the local people often ignored them, objected to them.

Well, this paper came out, and Henry Van Zile Hyde, who was in charge of the Health Division of the Institute of Inter-

American Affairs, came back from leave, and we became friends--he had supported me for a long, long time, I mean moral and other kinds of support--because of him, I was assigned to the fifth World Health Assembly in Geneva in 1952, as a so-called advisor to the American delegation. So I had a chance right from the beginning to become acquainted with important people, nationally and internationally, in public health.

A year after that we moved to Berkeley, where because of my contacts in Washington some of the people here had been told that I was coming--I think I told you that I got in touch with Bill Griffiths and Dorothy Nyswander. To make a long story short, in 1954 I started offering a seminar in public health education, the anthropological and cultural aspects of health education.

From then until about 1962 I held a joint appointment in public health--a courtesy appointment as lecturer in public health. So I went to their staff meetings, and I became acquainted with most of their regular personnel. Chuck Smith, the dean, was a good friend. I began building a wide network of friends who knew what I had done and appreciated what I had done. It was mutual. I came to have a very high regard for them.

International Cooperative Administration, Miniclier, Taylor

Foster: On the basis of the first seminar or two that I had given in the School of Public Health, I published a little article in *Public Health Reports* called "Guidelines to"--and for some reason, I added Community Development, not Health. And Louis Miniclier, who was the man in what is now the Agency for International Development, was in charge of a program in community development, and this caught his eye, and he got in touch with me and invited me to a workshop that was held at Endicott House, in Needham or Dedham, I forget which, near Boston. It was the Endicott Johnson Shoe Company. The old family mansion had been turned into a big conference center.

To make a long story short, because we talked about this earlier, once when I was back in Washington I checked in with Louis and he said, "I'm forming three teams this summer"--the summer of '55--"to go to several countries where they've had successful community development programs, and to write reports on them. Would you be interested in being a member of one of the teams?" I said, "I certainly would." And he said, "We ought to have an economist. Do you know one?" I said, "I do. Paul

Taylor is his name." And that's how Paul Taylor became interested in this consulting.

Riess: That wasn't your first meeting with Paul Taylor, then.

Foster: I had known Paul slightly from the time I was a graduate student, but never very well. I knew him, and I appreciated his work, but I really never got well acquainted with him until we spent those ten weeks together.

To round out our group, I also suggested Isabel Kelly for another team. I was responsible for, I guess, three of the seven or eight people all together that formed the three teams. Isabel Kelly and a man--I forget his name--went to Puerto Rico and one other Caribbean island. I forget which it was. Then there was Hugh Masters and another fellow who went to India with Paul Taylor, and Dick Adams and me. Then they came back and went to Egypt, I believe, and we went on to Pakistan and the Philippines. By then I was well on the way to being a consulting expert.

Riess: How did you know what you were looking for? How much direction had you gotten?

Foster: We knew we'd be met at the airport by U.S. representatives, and they knew why we were there. We were there not to evaluate them so much as to find out what we could learn, so that we could provide good guidelines for later people coming along.

Riess: They were considered to be successful programs.

Foster: They were among the better programs, yes. But it was implicit--I think they recognized we came as friends, not as critics. I think that's basic to any good consulting. You can't go in with a "Let's get the dope on the bastards" attitude and expect to have much rapport with the local U.S. personnel.

All the years I was in public health consulting, I think I managed to convey to the people I was working with that I was a member of their team. I considered myself a member of their team, not a critic, so I always got along very well with them. In most cases, I had a great deal of respect for them. They were very able people.

It's interesting to me to see how people with different backgrounds and different premises evaluate the same kinds of data and come to very different conclusions from those the anthropologist comes to.

Field Interviewing Skills

Riess: How would you characterize the difference between you and Paul Taylor?

Foster: Paul was as much an anthropologist as I am. I think I mentioned that he wrote an article on making pots, published in the *American Anthropologist* that I consider a classic. The skill of the anthropologist is that person's ability to gain the confidence of people, and to be able to conduct profitable interviews. This is very interesting to me: Paul was a magnificent interviewer. The three of us would sit down with an official, we'd take turns asking questions. And Paul, if he didn't like the person, he was all smiles, he'd lead him along, until finally he'd come in with his last question, and you just wanted to say, "Now, buddy, shake your head." [laughter] The official had not recognized that Paul's final, seemingly mild, question, was a slash with a straight-edge razor so sharp that the official had felt nothing, when in fact he had been beheaded!

I remember Dick Adams, who was a professor--milk sanitation was his great specialty at Indiana University Medical School--he said, "George, I don't understand it. You sit down with a group of informants, nurses, let's say, and in five minutes you're all laughing. You're having a good time. The time comes to go, and you don't want to go. I sit down and I tense up, and they tense up, and after five minutes I don't know what to ask them."

I realized then that anthropologists have mastered a technique of putting informants at ease and being able to follow through on questions. It gave me a renewed respect for my discipline. The number one skill is the ability to get good data by asking questions. Paul was superb at this.

Riess: Can you evaluate graduate students as to that ability?

Foster: I evaluate them on the quality of their notes. I'm not often there to see them in the field, but I've always insisted that they send copies of their notes. I have always emphasized to students the importance of good interviewing skills and note taking and keeping good notes and thinking about what they mean.

Riess: You wouldn't assume that their behavior in the classroom would be an indicator of their behavior in the field, with an informant.

Foster: After one has a student in several seminars, one has a pretty good idea of the student. Before they go into the field, they would

have been in their advisor's classes for at least two years, so one would have a good idea.

In the case of Margaret Clark, who was the first student that I really felt responsible for--Health in the Mexican-American Culture, you know that book? I saw her at work much more than I did most of the students. I met some of her informants, I'd go with her when she'd go to see informants. She was a very able person.

The students that I took to Mexico--I saw them at work, and they all did very well in interviewing.

Riess: Maybe Adams learned something in his short time with you and Paul Taylor. Maybe it's a behavior that could be mimicked?

Foster: He learned that it was a talent that he never thought about that was basic to certain kinds of occupations.

Riess: In these consultancies did you have theories that you were trying to find evidence for?

Foster: No. I was just going in to see what I could find out, and draw conclusions.

When I worked for the World Health Organization, part of my assignment usually was to talk to the people in the American Mission, as well as some of the local health educators. They'd bring up problems, and we'd have general sessions. I'd make suggestions as to what the problem seemed to me to be and ask them to go out and check. Sometimes I hit it on the head, other times, I didn't.

Riess: And you were there long enough so that there was some closure.

Foster: Yes, usually. I was three months in Afghanistan, and that was a catastrophe as far as anything being accomplished was concerned. I was almost six months in northern Rhodesia, in the same place, working with the same people. That was the longest period I spent with the same group. I was rarely there less than a month, though, often six weeks, so I was there long enough.

In the case of India, I went back several times, working with the same people, so that I had continuity and friendship and knowledge. It was very valuable. It was not this hit or miss consultancy in which you go in for a day or two, as the specialist telling them what they should do. I learned as much in that as I did in any of my formal field work.

Lessons in Basic Beliefs

Riess: You were encountering bureaucracy, and thinking about bureaucracy.

Foster: I did research on bureaucracies without realizing it initially. My service, being a government employee for almost ten years before I came back to Berkeley, in the Smithsonian, and then seeing the British colonial system in Northern Rhodesia, and the bureaucracies in the American AID missions, those all stimulated my interest and gave me the meat that I've used in talking about not only bureaucracy, but also for an area of work I've talked a lot about but never written very much about: the fundamental premises that underlie behavior. I don't know why I haven't written a book on that. I really should. Or at least an article.

For example, Mel [Melford E.] Spiro wrote a very interesting article on Buddhism in Burma, and the popular belief that Buddhists in that country lack a capitalistic sense. He pointed out that even wealthy Buddhists live in very simple houses, eat simple food, and appear to have very simple tastes. At the same time, they spend large sums of money on gold leaf to apply to images of the Buddha. This seemingly odd (to non-Buddhists) behavior, he says, is ascribed to their asceticism: they're simply not interested in the "good things of life."

Yet, he insists, they are interested in the good things in life just as much as you and I. The explanation for this behavior--irrational to Western eyes--lies in the difference in a fundamental premise, a belief about life, separating Buddhists and non-Buddhists. We westerners believe, and often flippantly say, "You only live once." Buddhists believe in reincarnation, that they live many times. Buddhists hope to be reborn in a higher plane than their current existence, and spending money on religious acts is the most certain way to obtain the merit that leads to better luck the next time. Therefore, Spiro argues, living simply and spending the money one saves by so doing is the wisest investment a good Buddhist can make. Their behavior is just as capitalistic as ours, given *this fundamental premise about reincarnation*. I have always been intrigued by this concept.

Another article that I read, by a professor of education at University of Oregon, and a Nepalese--the operational idea that I got from that is that all of knowledge is known somewhere. This was an article about the problems of teaching the scientific method, experimental and observational methods, to people who believe that all knowledge is already known. The way this turned out was--well, they asked informants about things like penicillin and airplanes, and they'd say, Well, yes, they're new, but the

basic principles have always been known. Birds fly. Things like that.

To me, and I think to most Americans, the idea of finding a guru at whose feet you sit and ask him to fill your mind full of his wisdom is ridiculous as a substitute for science. But when you believe that all knowledge is known, surely that's the wise thing to do if you want to get ahead: find a guy who knows what you'd like to know, sit down at his feet, and become his disciple.

There are all kinds of ways in which premises of which we're generally not aware are determinative of our behavior. In my consulting, which brought me in touch with a number of cultures and systems I would never have otherwise known, I've been very much advanced in my thinking by these experiences.

Riess: Not too much has been written in a comparative way?

Foster: It's something that a few psychologists have written about, but it's not something you find in anthropology volumes very much. Years ago people talked about world view, and an anthropologist named Morris [E.] Opler had an article about premises, which he called themes. They were pretty much the obvious ones, like "hard work will lead to success," and "honesty is the best policy"--things like that. These subconscious or unconscious premises are just not a part of the literature at all, as far as I can tell.

Riess: But how do you identify these subconscious premises if you're not aware of them?

Foster: I think it's a question of triangulation. You just keep your eyes open, and occasionally a bit of behavior will strike you as being weird, not fitting the paradigm that you expect, and you begin to think about it, and little by little it becomes more clear. But I don't know how you can teach it. I think some people have the knack for it, and others don't.

Riess: So, for a broader view--is there an international organization for anthropologists where you might talk about such premises, or questions?

Foster: Yes, there is the International Union of Anthropological and Ethnological Sciences, which meets every five years. It's good, because you meet with people you've probably corresponded with, or people that you haven't, but you're interested to find, but it's all at the informal level, largely, I think.

Riess: On your consultancies, were there different ways of working that you had to learn?

Foster: In the case of British Africa--Northern Rhodesia--I learned there were certain things that were expected. When you go into a different district, you report immediately to the district officer and let him know you're there, and you let him know when you're going to leave. There are niceties like that that are expected of you. The British themselves, when they've been on leave and come back, would go to the governor's mansion. They wouldn't necessarily see him, but they'd report back, and probably leave their card.

But as far as the working is concerned, an anthropologist works pretty much the same way, I think, whether he's working in an applied setting or a theoretical one. You're trying to get as many data as you can, and you want to find out what those data mean. In the case of a public health program, let's say the problem is why did the American type of public health center not initially draw the clientele you'd expect it to draw, if you're an American? That's more focused than most of the work I've done. But basically that simply means I ask questions that seem to me to be directly or indirectly pointed toward this kind of a problem, rather than toward, let's say--I was going to say rather than religion or agriculture, but it may be that religion and agriculture are relevant.

Riess: Did you get to meet the clients?

Foster: I often met the clients. In the spring of 1952 I spent three weeks in Chile with Ozzie Simmons, who was a sociologist on the Institute of Social Anthropology staff, and Grete Mostne, an Austrian refugee in Chile, a Chilean citizen. We went to the town of Temuco, where we spent two weeks going around with the public health nurses as they made their rounds, observing them, and then asking if we could ask questions, and then we'd ask questions. Once I attended a well-baby clinic at which the nurse was lecturing the mothers about how to take care of a well baby. After I was done listening I asked questions that seemed to me to be relevant. Yes, we had lots of contact with the people, themselves.

Introducing New Ideas, Change

Riess: Was your assignment basically to troubleshoot, or to report?

Foster: I was there basically with the interest of the organization in mind, to find answers to problems that were facing them. But I

find that anytime I find things like that, ultimately they have a theoretical importance.

For example, I have developed a hypothesis that has attracted a good deal of attention about the stages of recognition of the problem of introducing new ideas, which came almost entirely from my public health work: the idea that the first comprehension when technical aid problems got started was, "We'll give it to them on a silver platter." (I think we talked about this earlier.) We know what they need. We'll make it available to them, and they'll come rushing. They'll want it. We found out that was not true. That was the stage we were at when I first went to Chile. They were puzzled because the American-type public health center was not drawing personnel.

The second stage that came to mind was that in order to work effectively with people in helping them to change their beliefs, you've got to know what they already believe, so you can phrase your program in terms of what will be meaningful to them. That's where a lot of the programs are still stuck, because the problem is defined as out there with the people.

I think it was my experience in Northern Rhodesia that crystallized my thinking on the third stage. The problem is just as much the barriers to change in the innovating organization: time, the hours in which services are available, being arranged for the convenience of the staff, not for the local people. That's the one that they're the most reluctant to accept because that means "I've got to change, too," and it's harder to change oneself than it is to change a peasant community, I think. That's the example of how my applied experience has fed into theory.

Another thing that struck me was the premise, in Northern Rhodesia, among the British, that in order to be able to become a district officer, or a provincial officer, one had to start at the bottom and work his way up. You had to be out in the boondocks and learn everything that was done. This "work-your-way-up-from-the-bottom" syndrome interested me because when I was expecting to be a packing house executive, the idea in my family was that I would work for one summer in the smoked-meat department, and another summer in the machine shop, and learn everything about the system, so that when I got to the top, I'd know all the problems.

This premise is all wrong because in today's rapidly changing societies, by the time one gets to the top in, say, middle age, most of what he or she learned about the operation a generation ago is out of date. Not only is a person's idea as to how a department functions based on these memories of no help, but often they constitute a handicap when used to try to solve

contemporary problems. This is as true for would-be meat packers sixty years ago as for colonial officers thirty-five years ago. I think this is particularly a British premise, since my father (who started me out in this fashion) was much influenced by his English-born father.

Pottery

Riess: When working for AID, did you have time to go off and do your own investigations? Were you, for instance, able to look at pottery in these places and continue that interest?

Foster: To some extent. In the trip I made with Paul Taylor, I gathered a lot of data on pottery making in Bengal, both in west Bengal and east Bengal, in India, and in Pakistan [today Bangladesh]. I wrote a long article in--I think it was the *American Anthropologist*--called "Pottery Making in Bengal." In the Philippines that same summer I got to a pottery-making village. And I had always heard about resin-coated pottery, and by gosh, here they were coating pottery with resin. So I did a two- or three-page article, just a note on that, with a photograph or two. These Indian observations also answered, in my mind at least, a basic problem about which anthropologists have speculated since the beginning of time: Why is molded or coiled pottery made as much by, or probably more by women than men, yet when the potter's wheel appears in history it is always the work of men? When I saw the tremendous amount of strength and continuing energy demanded in starting and keeping rotating the huge, heavy Indian potter's wheel--really a flywheel--I realized that women on average simply do not have the necessary strength.

And I've also gathered a good deal of ethnomedical data in the course of my consulting, information about the local health beliefs and practices. So, yes, in addition to trying to see some of the major sights of a country, taking advantage of long weekends or holidays, or taking a week or two at the end of an assignment to be a tourist, I've always tried to obtain data that bears upon, that has comparative value with respect to, my major theoretical interests.

Riess: In your chapter in the Wenner-Gren Symposium on Ceramics and Man in 1961, you write that potters don't appear to value their work?

Foster: Yes, their low self-image. I have come to the conclusion, as a consequence of seeing pottery made in a good many places besides Mexico, that potters tend to have low self-esteem. They recognize theirs is dirty work. When I've shown interest, they've been

delighted often, because they say, "You're the first person [meaning from outside the community] who has shown appreciation for our work. You recognize the artistic value of our vessels. Most outsiders disdain [nos desprecian] us and our work."¹

There's a young man in Tzintzuntzan named Manuel Morales-- those are examples of his work there [looking at a kitchen shelf], the blue and the reddish brown--he's widely recognized as a superior potter, and he has no doubt that he's a competent person. He gets good prices for his work and sells in this country and all over Mexico. But it's like the United States: a good potter is an artist, but the home potter in the U.S. who just pots away is just having fun.

Riess: Of course, the home potter who pots away in Tzintzuntzan is making a pittance.

Foster: Pittance is the right word.

I've asked other people--outsiders who try to help potters-- how they look upon potters, and that's very interesting, too. In Nepal, I remember, the community development workers with whom I was working were rather scornful of potters. They also said they are very conservative; they are the most difficult people to change. And I've found that generally to be true. That's a conclusion I've come to, not so much from potters themselves-- except in Tzintzuntzan, where they are slow to change, the vast majority at least--as from the people who work with them, whose concern is to help them change.

There's plenty of theory out there. And I've often said that as many of my theories come from the applied work as from the purely theoretical work.

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Foster: [later in the interview, describing basic pottery techniques] There's just taking a lump of clay and molding it. There's spiral, there's coiling, there's modeling. And then there's a finishing technique that's quite rare, called paddle and anvil. In order to set the clay, the potter either with a fist on the inside or a stone, and a wooden paddle on the outside, goes around paddling the soft clay. That's the article that Paul Taylor did ["Making Cantaros at San Jose Tateposco, Jalisco, Mexico," *American Anthropologist*, 1933]. I once went to San Jose Tateposco to see it, and by gosh, they were doing it. And once I saw that same technique used in the Pacific.

Riess: "Dirty work"--is that expression associated with pottery, no matter the culture?

Foster: It refers to your fingernails, the grime and dust and powder and dirty clothing, muddy clothing. The weaving of wheat straw, for example, is not dirty work because you can be in your Sunday best and do that if you want to. It just means that to make pottery is, in the literal sense, a dirty occupation, like digging ditches.

Riess: How did you and Mickie decide from whom you would buy pottery?

Foster: Most of the pottery we have from Tzintzuntzan has been given to us by friends. We bought a piece or two from Manuel, but a lot of these are presents he has given us. He was here in this house once, and he brought us one of those pots as a present. Later we bought several from him for friends. We've really bought almost no pottery in Tzintzuntzan. It's all been given to us by friends.

Riess: Would you have to be careful not to be partial to someone's work?

Foster: No, if I wanted something, I'd buy it. But there are friends we've helped in various ways, and that's the way they'd reciprocate.

Berkeley Faculty Seminar in Community Development

Riess: In 1958, there was an interdisciplinary faculty seminar in community development that Paul Taylor pulled together on campus.

Foster: That went on for about five years. Community development--Paul had not had any formal contact, I think, with the community development movement before he met Louis Miniclier. Miniclier and Paul became bosom pals. They respected each other enormously, for good reason.

Miniclier thought that it would be desirable to have a prestigious university organize a seminar at which the leaders of the national community development programs could come and receive high-level university training. Paul thought that was a great idea. I thought it was a good idea, too. But Paul is the one that took the primary responsibility. And we had about three different groups, as I recall, that came for periods of up to about six months. I don't think we ever gave them any kind of diploma, which was a mistake. I thought we should have at the time--maybe give them--a certificate or something.

It was a lot of fun because some of the people we met on our trips were among those that came to this country. They were great fun, the Africans, particularly. I have photographs of taking them over to Marin County, Tamalpais, picnicking there. Paul hired specialists. Jack Mezirow was one. David Brokensha was another.

Riess: David who?

Foster: Brokensha, B-r-o-k-e-n-s-h-a. He was a South African who was unusual in that he was highly sympathetic to the blacks. In fact, I think he was unable to go back to South Africa. He ultimately went to Santa Barbara as a professor and has only recently retired there.

Jack Mezirow was a formal community development trainer. He was hired by the university, as was David Brokensha at that time, for these programs that Paul Taylor had organized.

Riess: How much time were you able to give it, since you were--it seems to me you were pulled in many directions.

Foster: I was. I gave a lot of time. I think I gave twelve hours of lectures in the fall for about three falls, on top of my regular assignments. And there were lots of Saturdays and Sundays devoted to taking people around and entertaining them in our homes.

Riess: For doing this kind of work, were you given a reduced teaching load?

Foster: No, I had no reduction in my regular teaching assignment.

Riess: How did you move from AID to WHO?

Foster: I made three trips to Indonesia for the Western Consortium on Public Health, which was the School of Public Health in Berkeley, I think the Medical School in San Francisco, and several other West Coast schools that had a joint grant from AID to help Indonesia with its health problems. I had had a fellow in my class named Ray Carlaw, a very interesting New Zealander who became an Australian citizen and studied public health in Australia, and for a number of years had run health facilities among the fuzzy-wuzzies in New Guinea.

Riess: The fuzzy-wuzzies?

Foster: Well, that's my term. He didn't use it, but that term was often used by the Westerners for the natives of New Guinea, for their great fuzzy hair.

He took a D.Ph. in health education with Bill Griffiths. Dorothy Nyswander had retired by then, and I don't remember whether Beryl Roberts was still active or had died. One day Ray Carlaw came to me, and he said--this was about 1972--he said, "Do you know any anthropologist who would be interested in making several trips to Indonesia to advise on health education?" [laughs] It didn't take me long to say, "I'm one who'd be delighted to do it." He said, "I hoped that was the answer I'd hear from you."

So I went out to Indonesia three times for this Western Consortium, which was AID-financed. While I was there, I met a Sri Lankan named Piyaratna who was in charge of health education for the regional office based in India, the South Asian regional office. He liked what I was doing, and he asked me if I would like to come to India and be involved in health education. That's one way--the way I got into WHO there.

I also got into WHO at a different level. About January, 1976 an official of UNICEF in New York named Ed Lannert asked Ben [Benjamin D.] Paul, an anthropologist at Stanford, who had edited a book called *Health, Culture, and Community*, published by the Russell Sage Foundation in 1955, if he'd be interested in working in a UNICEF-WHO project on primary health care. Ben was the logical person to consider for the assignment because his book had had a tremendous impact among people working in the field of international health, pointing out so clearly what anthropologists had to offer. Ben was not interested, but he said, "I can tell you somebody who might be." And he gave my name.

Lennert got in touch with me, and I said, "Yes, I'd be interested." So in 1976 I made three trips to Geneva, for a total of about two months, as a consultant to the "1977 UNICEF-WHO Joint Committee on Health Policy Study on Community Participation in Primary Health Care Activities," working on a report that was a major point in WHO's plan to achieve "Health for All by the Year 2000." It was one of the early idealistic WHO programs, emphasizing health rather than illness.

I became acquainted with a number of people in that way, so I had a couple of leads in WHO. And another student I had had in the UC School of Public Health, a medical doctor named Nizetic, was program planning officer for WHO's European Regional Office, in Copenhagen. He invited me to come to a couple of conferences in Europe. So my ties to WHO came about in various ways, always at the personal level.

Riess: Interesting that so much came from your students.

Foster: It is, yes. I learned from this that you don't just apply to WHO and say, "I'm an anthropologist. I'd like to advise." You've got to know somebody who knows you and asks you to participate. Once you start building a network, you--.

Well, I'll give you another example. Once when I was out in India on a health education project, I was at a dinner and I sat next to a young German veterinarian named Fritz Käferstein who was heading up a new WHO program called "Food Safety." I told him I was in health education, that my field was anthropology. He said, "What in heaven's name does an anthropologist offer health education?"

I told him a little bit about what anthropology was all about, and his eyes popped wide open, and he said, "Why, that's exactly what I need in our Food Safety program." So for a couple of years I was deep in food safety. I helped him organize a big seminar in Bangkok, and we made quite a splash for a couple of years.

Riess: AID sounds like community development, and WHO sounds like health, yet they're both public health.

Foster: Yes, in the sense that both organizations were concerned with preventive medicine. Community development, which includes a variety of health-promoting activities, was a major unit of AID. At one time in the seventies it hired more anthropologists than any other unit of government, probably than any other bureaucracy in the world.

I published an article in 1962 titled "Community Development and Primary Health Care: their Conceptual Similarities" in which I drew attention to the history of the two movements. Community development appeared on the scene in the late 1940s--the same time WHO was established--and it wasn't until a generation later that WHO latched on to the idea of working with villagers in searching for answers to basic problems of sanitation, pre-natal health care, immunization and the like. One would have thought that WHO papers and workshops would have referred to the earlier work. But not one word about the formal community development experiences appears in the WHO literature.

That's the way of bureaucrats: no one wants to give credit to people and organizations that have dealt with what you are selling as a new and innovative project. If you can cast old ideas in new terms, then they are yours, and you get the credit.

Riess: "Health for All by the Year 2000." Did you think it was possible?

Foster: No, it was not possible, and nobody believed it was possible. The most charitable interpretation is that it's good to set utopian goals that will push organizations to do their very best.

WHO is a very political, patronage-based organization. Everyone that I know who works there is of the opinion that it's politicized to an extent--and to a greater extent every year, I believe. I have written a couple of articles on this phenomenon in which I point out the dynamics of it as I see them.² WHO's social research is not designed to find out answers, it's designed to justify what's been decided upon as policy, and the "research" results are presented in such a fashion that they prove the policy is the correct one.

Riess: I can't imagine that you could write such articles without being dropped from the A list of consultants.

Foster: Well, it was toward the end of my career. [laughter] Several people commented on it. They said, "We're waiting until we retire before we can write something like it."

The Medical Team

[Interview 9: February 24, 1999] ##

Riess: When you were consulting with AID and WHO, did you go out on teams with medical people?

Foster: Often. In my first work in Latin America, I was a member of a team that was directed by Wilton Halvorsen, who was chief of public health of the State of California at the time--this was in 1951-52. He was the head of a group of medical people who made an evaluation of the first ten years of the bilateral health programs of the Institute of Inter-American Affairs. That team included such people as George Strode, who had been high up in the Rockefeller Foundation. There was a statistician, there was a public health nurse, there were a couple of doctors. That was my first major contact with the medical profession other than as a patient. I began learning a lot about the medical profession.

²"World Health Organization Behavioral Science Research: Problems and Prospects," *Social Science & Medicine* 24, 1987, pp. 709-717.; "Bureaucratic Aspects of International Health Agencies," *Social Science & Medicine* 25, 1987, pp. 1939-1948.

Since then I've believed that applied research is just as productive of ideas as purely theoretical research. That experience was what got me interested in the structure of bureaucracy. It was the first time I'd been exposed to a radically different bureaucracy from what I have previously known. And that's the way anthropologists work. We're exposed to something--in fact that's the justification for anthropology, I believe--when we see something that's drastically different from what we know, we try to relate it to ourselves and see what the similarities and differences are.

Riess: That you have to be so sensitive to your surroundings is an alarming responsibility.

Foster: I don't see anything alarming about it at all. I think it's wonderful.

I think anthropological field work is as much an art as it is a science. I think it's simply the sensitivity of the investigator, his alertness to unusual situations, that's productive of ideas. In this case, I knew about bureaucracy. I had read Max Weber and the like, but it never meant much to me until I was in a situation where I was working intimately with people whose assumptions were different from mine and who had a structure that was easily visible from my perspective. And that sparked my interest in bureaucracy.

Later I had an opportunity in what was then Northern Rhodesia, the British colony of Northern Rhodesia, to look at another bureaucracy from the standpoint of an American.

Riess: What I meant earlier was that the anthropologist has to be so sensitive, so constantly "on," so alert, like an animal almost.

Foster: I don't believe that I'm that way. I think it's just that every once in a while something happens that penetrates my thick head, and I realize, "Here's an interesting topic." I'm not aware of being constantly on the alert, asking myself, "Is this something I've never seen before?" When it becomes critical, I don't have to be told, it tells itself.

Riess: Was there anything particularly alarming or surprising to you about the orientation of the medical people?

Foster: No, I liked them. They were a high-class bunch of people, well educated, and they knew what they were doing. And they liked me, I think, as I liked them. I recognized they were learning things from us, the anthropologists, who were members of the team. Ozzie Simmons in Peru and Chile, Isabel Kelly in Mexico, Kalervo Oberg

in Brazil, Charles Erasmus in Colombia and Ecuador--they all came in at different periods of the six-month expedition. We worked well together. We got along very well.

Traditional Cultures and the Impact of Technological Change

Riess: What you were doing, in your role as consultant for AID and WHO, did it become much richer over the years? Or was it a routine, pretty much the same protocol in each country?

Foster: I was always developing my own ideas. I started out believing that it was desirable to change people's behavior and to get them to accept contemporary Western medicine, and little by little I came to see that I was guilty of deciding what other people should do, and they had ideas of what they should do themselves. So, no, I wasn't doing the same thing, my ideas developed as I went along.

I was initially not much interested in the workings of bureaucracy. That's something that came along in midstream as I realized that I had a marvelous opportunity to--that I was participating in a series of bureaucracies, so that I could compare them and winnow out the common factors.

Riess: And this all got incorporated into your teaching work?

Foster: Oh, yes. I heavily laced all of my lectures with my experiences. Nothing interests students as much as when the professor can talk about what he's doing for research, and anthropological research is particularly interesting to students because you're out in the field and you've got funny anecdotes of experiences you've had.

Riess: *Traditional Cultures and the Impact of Technological Change*, was that book used by the Peace Corps? And how? Did you consult with Sargent Shriver and people like that?

Foster: No. You see, there was no centralized planning for the Peace Corps training programs. They gave contracts to universities, and each university did it any way it wanted. My longest period was two weeks at Rutgers, in 1960 or '61, with the first group training to go to Colombia. Then I think it was at Los Angeles State University, which had a shorter program, where I was next a consultant. There were three or four, maybe a half a dozen altogether where I taught, but they were all individual. None was organized in Washington. There was very little exchange of information from one school to the next, as far as I could tell. I thought the training was not very well organized. But I wasn't

prepared to devote a lot of time to getting it done my way--
[laughs] I wasn't even sure I was right.

Riess: But your text would have been *Traditional Cultures*--.

Foster: Yes, that's the best-selling book I've ever written. It sold, I guess, well over 100,000 copies. It was translated into Spanish, Portuguese, Dutch, and Farsi. I don't know how many were sold in those languages.

Riess: And who was using it?

Foster: I guess any person who was interested in international work and the problem of working with people in other societies--societies with exotic customs, from the point of view of most Americans.

Riess: In the introduction to the book you talk about the "need to know cultural forms to understand resistance to the new," and the "need to consider the ethical problem in deciding what is good for someone else."

Foster: That was my applied anthropology course until the book came out. After that, I had to change my course. I couldn't just go on.
[chuckling]

Ralph Linton was a well-known American anthropologist who published a book in 1936 called *The Study of Man*. I remember talking with him about the book, and hearing him say, "That was my introductory course, I had to write a whole new course after the book came out."

After *Traditional Cultures* was published I faced the same problem: I had to work out an entirely new course. And this "new" course came out as my 1969 *Applied Anthropology*. Then I was again faced with the same problem. This time I emphasized health and medicine more and more, and a good deal of this version appeared in Barbara Anderson's and my 1978 *Medical Anthropology*. By retiring the following year I avoided the problem of working up new materials into a full blown course.

Long-Term Studies

Riess: Does your book about long-term field research fit into the same continuum of texts?³

Foster: No, not so much. That came about in an interesting way. Hussein Fahim was an Egyptian who took his Ph.D. with me. And Ted [Thayer] Scudder, is a Harvard Ph.D. who has worked with Elizabeth Colson in studying the native peoples of the Gwenbe Valley, a stretch of the Zambezi below Victoria Falls, for more than forty years. At the time Elizabeth was in charge of the Rhodes-Livingstone Institute in Victoria, and Ted had come out to Northern Rhodesia in the mid-1950s as a graduate student and joined Elizabeth in the field. They've followed the Gwembe Valley people ever since.

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Foster: In 1970 I went to Egypt to attend a long seminar having to do with desalinization, using atomic power for desalinization.

Riess: Why were you invited to that?

Foster: I suggested we have the seminar, I guess that's why. Laura Nader's sister, Claire, worked for the Atomic Energy Commission, and through Claire I was put on an advisory committee for a desalinization project the commission was interested in. We met in Nashville, Tennessee, several times a year. The idea was to try to get Egypt and Israel jointly concerned with desalinization, and that would solve the tension between the two countries. It was pie in the sky, if there ever was such. But the Atomic Energy boys were always looking for justifications for the expense of their work.

After attending several meetings, I suggested that what we needed was really a more general seminar, going for a couple of weeks, at which we'd have a broader group of people and consider all the aspects--the social as well as the technological. The Atomic Energy Commission, or the relevant branch, agreed that it was a good idea, so they set it up in Egypt, where Ted Scudder had worked. He came out also. And Hussein, I forget why Hussein was in Egypt at the time.

³Long-term Field Research in Social Anthropology, co-edited with Thayer Scudder, Elizabeth Colson, and Robert V. Kemper. Academic Press, New York, 1979.

In any event, to make a long story short, Hussein and Ted Scudder and I had gone up the Nile to see some of the displaced Nubians that Hussein had studied. We were sitting on the balcony of the Old Cataract Hotel in Aswan, and as we talked the idea came to us. We realized that we had been studying the same people for from ten to twenty-five years. And we were sure that this--the long-term study of the same people--had never received formal attention. So, on the spot we decided that we'd ask Wenner-Gren for support for a symposium at "The Castle," Burg Wartenstein, on long-range studies in social anthropology. So that's the way that got started.

Hussein was present at the symposium, but he was not involved in planning the conference. Elizabeth Colson was pulled in. Van Kemper, who did a great deal of the legwork, was sort of the secretary of the meeting--we decided it would be more honest if we made him the junior editor. That was a very good study, I think.

At the Castle, Wenner-Gren Foundation

Riess: What is Wenner-Gren? Who is Wenner-Gren?

Foster: Axel Wenner-Gren was the Swedish match king. He was a Nazi. During the war--maybe before the United States got in the war--he realized that he had to do something to clean up his act a bit. As to why he thought that anthropology was the way to do it, I don't know. But there was a Hungarian doctor named Paul Fejos, F-e-j-o-s, who was the ideal man for the job. Axel Wenner-Gren gave him the money to set up a foundation to do anthropological research and to hold meetings and things like that. It was called the Viking Fund at that time, for the first ten years or so.

Riess: Was that because it sounded less German?

Foster: Yes. It was only later that his name was attached to the fund--it became the Wenner-Gren Foundation.

Paul Fejos set the pattern for these conferences. After the war, the foundation acquired this castle on the outskirts of Vienna, when the dollar still was worth a lot. Those were legendary meetings. Lita Osmundsen, who later married Paul Fejos, was the person who really ran them. After he died, she was the president of the foundation.

They'd go on for about ten days. There'd be, oh, at least two a month or so, for five or six months a year. Anyone could propose a seminar. The first one that I went to in '61 was Ceramics and Man, proposed by Fred [Frederick] Matson, who's a ceramicist-archaeologist. The castle was fixed up with a big seminar room, good music. The stable was converted to a dormitory, where most of us slept. There was an old ruined tower which had no roof and had nice grass at the bottom. A night or two during the conference there would a symphony on the loudspeakers, and you'd have a blanket and a chaise longue, and you'd stretch out and listen to this music. It was fantastic.

Halfway through the conference, when people were beginning to get kind of mad at each other, a day was always taken off for a tour of Vienna. So the meetings were well organized and well worked out. Those were great things in the whole life of the--I don't know of anything that played a more important role in American anthropology, world anthropology, actually--than the Wenner-Gren conferences.

I participated in three. The second one was in 1967. It was called "Teaching of Anthropology in Latin America." That was set up by Johnny [John V.] Murra, and maybe Sol Tax--I can't remember. The third was ours on Long-Range Studies, which was very successful.

Riess: Halfway through, when people began to get mad at each other--it was a given that there was going to be a breakdown at midpoint?

Foster: Yes.

Riess: Tell me what that's all about, as a good observer.

Foster: You should ask Lita Osmundsen. She's the one who worked it out. People come in, and they're all jolly, but as their interests begin to get questioned and the tension begins to build up--Lita figured out that if they took a day off and went and did other things, that broke the strain and the participants could come back and work with a fresh approach and bring the thing to a conclusion. It's marvelous the way she had it sized up just exactly right.

Riess: What was the size of the group usually?

Foster: Oh, twelve, maybe fifteen at most.

Riess: Did wives come?

Foster: Normally not. In 1961 Mickie spent one night by accident. She was in Vienna with our daughter, Melissa, and a friend of Melissa's. It was about two nights before the end, and Paul Fejos said to me, "Where is your wife, George?" And I said, "Well, she's actually in Vienna. I'm going to meet her at the end of the conference." He said, "You're in a room in the stable with two beds, aren't you?" And I said, "Yes." He said, "Invite her out for tomorrow night, and we'll have a *Heuriger*." (The *Heuriger* is the music that they have in Grinzing, a festival at the time of the new wine.)

Riess: Singing?

Foster: And dancing. There are about three musicians, local people, who'd come and play. They'd sing, and if you knew--mostly the other people danced. So Mickie--that was typical of Paul, he was a real gentleman--she had a wonderful time.

He said, "We'll put on a *Heuriger*." Normally there's only one *Heuriger*, occasionally there's two. We'd had our two, but he said, "She's got to have a *Heuriger*." So that became known as the three-*Heuriger* conference.

Riess: Now, does it still continue in this style?

Foster: No, the castle was given up fifteen or more years ago. It became so terribly expensive. Now they have the same kind of conference, but they have it in different places. Our daughter, Melissa, was at one in Jamaica, I believe it was. A number are held in this country, and European countries, but they are held in different areas, different parts of the world. The investment in the castle was just too great to justify when the dollar went way down.

Mickie put on one of the last ones in the castle in 1977 on symbolism. She invited Stanley Brandes to help with the organizing of the meeting, and he is junior editor of the volume that resulted from this meeting. [*Symbol as Sense*, Mary L. Foster and Stanley H. Brandes, eds., 1980, Academic Press.]

I've always been glad that Stanley Brandes and Van Kemper were able to know the castle. The anthropologists who have been to a seminar there form kind of an elite. There was a definite break when they gave up the castle, and none of the conferences are in the same emotional category, I think. I've not been to any subsequent to that.

Riess: The same emotional category?

Foster: I guess that's the word.

Riess: Sort of a bonding?

Foster: Yes. If you've been to the castle, you've had an experience that relatively few people had, and you know it.

The jet plane brought about the change in meetings. Until the jet plane came, an overseas experience was by definition a remarkable thing, when it took twelve hours from New York to get to London, as against nine hours or ten hours from San Francisco to London now.

Riess: Did people at Berkeley envy you? "George is always running off"?

Foster: I didn't have more than my share, I don't think.

Desmond Clark and Sherry Washburn, the biological anthropologists, the pre-historians, were the ones that had more conferences than anyone else, in the Wenner-Gren series, the Leahey Foundation. I'd say Sherry and Desmond have been to seven or eight conferences there. At the time I was consulting, I did probably more consulting than anyone else in the department, but I don't think I was particularly envied for it.

X SUMMING UP

Medical Anthropology, The Discipline, the Book

Riess: When did you begin to think about medical anthropology as a new discipline?

Foster: Medical anthropology didn't really become a new discipline until ten years after our Smithsonian research in 1951-52. Until the early sixties what we now refer to as "medical anthropology" was called "Applied Anthropology in Medicine." That was the title of the paper by Bill [William] Caudill in the volume *Anthropology Today: an Encyclopedic Inventory* [1953, University of Chicago Press], the collected papers presented at a huge Wenner-Gren-sponsored international symposium held in 1952.

The terms "medical anthropology" and "medical anthropologist" do not appear in well-known journals until 1963. Sociologists had coined the term "medical sociology" a few years earlier, and I think anthropology took its cue from that field. It is a much more realistic term than "applied anthropology in medicine."

Riess: Is this a hazy area, between medical anthropology and medical sociology?

Foster: No, I don't think it is. I think I told about Virginia Olesen, the medical sociologist at UCSF--the two of us held a series of meetings in which we discussed the similarities and differences. We assumed that we were going to come to the conclusion that we were doing the same thing, that we simply came from different backgrounds. We were both astonished to find that there's a very sharp distinction between the two fields. The simplest way I can put it is that medical anthropology stresses culture, and medical sociology stresses society.

I saw that very clearly, initially, with our work in South America with the Institute of Inter-American Affairs team when I asked the members of the group to "find out what you can from observations why these programs are not performing as well as they would like them to perform." Ozzie Simmons, who was trained in sociology by Talcott Parsons--although he had a lot of anthropology with Clyde Kluckhohn--concentrated on the social structure of the health center. And the anthropologists concentrated on the beliefs, the medical practices of the people. I think that was very clear.

We did a pair of joint articles, ultimately published in *Social Science & Medicine*, in which we discussed this. The initial issue came out as a pair of articles in the *Medical Anthropology Newsletter*, in 1974. The difference is not only the subject matter but the methodology--the sociologists largely used statistical methods, while the anthropologists largely used participant observation.

Riess: There must be sociologists and anthropologists who push the definition. Don't anthropologists sometimes gather statistics?

Foster: Oh, yes. And some of the best medical anthropology has been done by sociologists, I think. Howard Becker, et al, *Boys in White; Student Culture in Medical School*, 1961, is really anthropology as much as sociology. And Everett Hughes, a distinguished Chicago sociologist, did awfully good work that I call anthropological. But in spite of these areas of overlap, I think the two fields are truly distinguishable, and I think they're recognized as such.

Riess: How did you go about co-authoring your *Medical Anthropology* text with Barbara Anderson?

Foster: I'd known Barbara Anderson when she was an undergraduate at Berkeley, about 1955. She was in my course on Europe, in which I talked a good deal about the *compadrazco*, the godparent system. She was of Italian extraction, in spite of her name, and she recognized the *compadrazco*. She did original research on this topic among Italians in San Francisco and did a monograph on the basis of her research. Not only did she do a monograph that was published in the Kroeber Anthropological Society Papers, but also a major article in the *Southwestern Journal of Anthropology* which, next to the *American Anthropologist*, was the best place to publish.

She was Barbara Gallatin, I think, at the time. Then she married Bob Anderson, a fellow anthropology student, and they went to Europe. She took her doctorate at the Sorbonne while they were doing research in France. She worked with Margaret Clark, who was the first Ph.D. that I felt responsible for. I think Margaret's

Ph.D. dissertation, which the Cal Press published, *Health in the Mexican-American Culture*, is possibly the first Ph.D. dissertation in the field of medical anthropology.

Barbara was, for a number of years, with Margaret at San Francisco, doing medical anthropology, so she developed medical anthropology interests on her own, not directly because of me, but we remained on good terms all during the years. She taught at Hayward State for some years before she was invited to go to Dallas, to Southern Methodist. So we'd see her a good deal, Mickie and I. Mickie was her colleague, of course, at Hayward.

We got to talking about 1976 or so, and we thought collectively we had had enough experience so we could do a medical anthropology book, so we sat down, and that's the result. It was quite a successful book for a few years. Every year we still get requests--it's out of print now--but we get requests to reproduce sections of it for classes.

Riess: How did you co-author? That's interesting to me, how a book gets divided up between people, how that works.

Foster: It ain't easy, I can tell you that. We took the parts we thought we knew best. She had done aging and psychological anthropology, so she was primarily responsible for those sections. I have been interested in ethno-medicine more than she had, and I probably knew more about the history firsthand than she did. So we'd write chapters, and then we'd rewrite each other's chapters until we finally came up with a homogenized style that satisfied us.

Riess: Did you have an editor who then homogenized it further?

Foster: No, not very much. We didn't have a great deal of editing done. Probably it would have been better if we'd had more--although I don't know.

Riess: In your earlier books, had there been an editorial hand, or have they been mostly run as written?

Foster: Mostly run as written. *Traditional Cultures and the Impact of Technological Change* had almost no editing. It had a good editor, but he had a light hand. He's my idea of the right kind of editor. He didn't change phrases because he thought they sounded better to him. He let me make my own mistakes if they were in any way acceptable.

Riess: You have a chapter in there on bio-ethics. That book came out in the seventies, and had that been on people's mind? A hot topic?

Foster: It was just beginning, I'd say, in the early 1970s, as I recall. It was assumed that doctors could make almost any experiments they wanted on patients. It was kind of taken for granted it was for the good of humanity. So it was a new field at the time, I'd say. Some of our graduate students were becoming interested in the topic at the time. I don't recall what the sources were. I don't even remember whom we quote.

Favorite Role, With Students in the Field

Riess: I want to have you look at your various roles and talk about that. I think of at least three roles: you in Tzintzuntzan, you as an international consultant, and you in Berkeley, teaching and advising. Maybe there's a kind of real-life you that's the fourth version.

Foster: Maybe a third of each of those.

Riess: I'm interested in which roles are really most comfortable and most natural. To what degree you're conscious of how you change.

Foster: I felt comfortable in all three of them. I enjoyed them all. I recognize their differences. Perhaps the field is the one I most enjoyed. When I'm on close terms with the students--. When I'm in Berkeley with students, particularly beginning students, I was fairly formal, and I had a reputation for being a bit cold and a bit distant, I think, among students--many of them, at least the ones that didn't know me so well.

So when I would get to the field and we were all working on the same problems, I asked them to call me by my first name--I didn't ask them while we were in Berkeley in those days. So that let down the bars for them and me, too. It was an easy--it was a relaxation, a vacation almost, you might say.

Riess: For them. For you?

Foster: For me, too, yes. I mean, to be able to associate with students who were just as bright as anyone you could want, on more or less the same level, and to be as interested in their data as they were--it meant a great deal to them and a great deal to me, I think.

Riess: And when you're in the field you're with colleagues, not just transported completely into a Mexican peasant society.

Foster: Sometimes I am, but often I'm with the students--I was, in my active years. And that's why I enjoyed going down last summer with Stanley Brandes's two students: Alison Graham-Yooll and Peter Cahn. They had grants to do their first field work, to get preliminary experience in the field.

Stanley and I went down in February, 1998 and made arrangements for their living, and then I went down with them with Mickie at the end of May for a week and got them started--introduced them to a number of our friends and got them started on their work. It was very satisfying to see how well they did, and to hear from these friends how much they liked them. When we were down in December, for Christmas, we found that everyone just loved them. I think I've always been successful in taking students who have gotten on well with the people. That gives me tremendous satisfaction. The students I take have made excellent contact with the people and are liked by the people and like the people.

Riess: You must have to make judgments about personality, especially if there were four students who wanted to go, and room only for two.

Foster: I decided who I would take with me. I took my first group of students in '59, and the last about 1970 or so, maybe a little later.

There are not so many that want to go. First of all, there's the question of money. Then, they don't speak Spanish, many of them, well enough to really consider them. I didn't run it as a formal field school, I'd just take a handful of students, I think four, at most. And I didn't have them all in the village, by any means. The first summer there was a young graduate student named Bill Iler working in the village. A second, Bill Smith, was on the island of La Pacanda with his wife and four-year old daughter. Cynthia Nelson was on the island of Yunuén, between Janitzio and La Pacanda. And Jean Cooke was in the mask-making village of Tócuaro.

There was a Guatemalan young man and his American wife, Pepe Monsanto and Dorothy, who were not members of my group, but they were down--I think Pepe was in the school of education. They had an apartment in Pátzcuaro, so we'd meet with them in their apartment once a week and have a copita or two, a drink, and talk about--everyone would tell what they had done, and we would exchange information and comment on our findings. We called it "Pepe's Pad."

Riess: Have you created a line of students? Like Kroeber and Lowie, and Boas, is there a Foster line?

Foster: I don't think so. It's common now for professors to take students with them to the field, in every university. Desmond Clark has taken a great many of his students to Africa with him, and Laura Nader does the same.

Riess: Will Tzintzuntzan continue to receive students from Berkeley?

Foster: I hope so. I think it will, yes. And as far as Tzintzuntzan is concerned, I would hope to be recognized as the father or the grandfather of the group.

Professor and Administrator

Riess: That role, the field work, was certainly satisfying. How did you see your role at Berkeley?

Foster: More formal. [laughs] I suppose I had an image of what a professor should be. I remember I always wore a jacket and tie when I lectured. I think toward the end, on a hot day occasionally I came in a sport shirt, but I never wore jeans to lecture. In fact, when I was a graduate student, I was embarrassed once--I think I mentioned this--when I had been out someplace and barely got to Lowie's seminar at four in the afternoon. I didn't have a jacket and tie on for his seminar, and I was embarrassed to death.

At Northwestern I always wore a suit or jacket and tie. It was quite a formal, dressy school in those days. California was more laid back, sartorially speaking, even then.

Riess: At Berkeley you had committee and chairmanship work, not exactly another role, but did you enjoy the administration work?

Foster: Yes, I did enjoy the chairmanship, though I was glad when I was not chairman. I enjoyed it because I found I could do it and was pretty good at it. I had had experience in the Institute of Social Anthropology; when I had turned the Institute of Social Anthropology from straight research and teaching to medical anthropology, I think that was the first time in the history of anthropology that a whole program had been oriented toward health problems. I was glad that we were able to carry it off.

Johnny Graff was the assistant or the associate secretary of the Smithsonian, next to Alexander Wetmore. He was the one I dealt with most. I knew that he had a very good regard for me, and I appreciated that. He thought I was imaginative in that

work. In fact--and I've never told this to anyone except Mickie-- he wanted to put me up for the secretaryship of the Smithsonian, but I didn't have the ability, and I knew it. I thanked him for his confidence.

Riess: That's a great honor. He thought you could do it, but you didn't?

Foster: I didn't have the imagination, and I knew it. I knew I could be a good professor and a good administrator at a certain level, but I felt I didn't have the imagination for a vast enterprise like that.

The next secretary had no more than I would have had, but he was there only five years, and then Dillon Ripley came in, and he's the one that revolutionized the whole Institution. Mike [Ira Michael] Heyman told me that Ripley created problems for the secretaries that came after him. He had established all of these new museums, but had left the problem of their budgets to his successors. Mike said that he spent a great deal of time and energy scratching up money to keep Ripley's new museums running.

Rapport and Long-term Work

Riess: The consultancies abroad, was that another face in the mirror?

Foster: It presented somewhat the same kinds of challenges in establishing rapport with people who, for good reason, often suspect anthropologists as being critical, as not having anything to offer.

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Foster: I recognized that I wouldn't get anywhere unless I was sympathetic to their goals and understood them and was tolerant of them. And I usually did approve of what they were doing. They were well-educated, nice people, just as clever as I, and just as dedicated, most of them.

One of my most interesting consultancies was in Afghanistan for three months in the late winter and the spring of 1957 for what ultimately became the Agency for International Development, the American Technical Aid program. I was a consultant in community development. I spent three months in the AID guest house. Most of the people were there for a shorter time, but some were there as long or longer than I was. So it kind of seemed like a club. We ate together, and we'd be out in the field, come back and find friends there.

I remember they gave me a wonderful going-away party when I left. It warmed my heart. They had a big sign up saying, "So long, Doc." They called me "Doc" for some reason, many of them.

When I was there I had an interesting experience. There was a man from the Kellogg Foundation, a medical doctor whom I had met in Washington five years earlier, when I was first in medical anthropology, named Ben Horning. He was about sixty-five, I'd say, a nice fellow. He had been an international specialist in health and I think the foundation was sending him on a final trip, just to keep him occupied, because there was no particular reason for him being there. He was there about ten days or so.

I was on a first-name basis with him, but once he said, "I envy you, George, because everyone calls you George." He said, "Nobody calls me Ben anymore." I thought, "You poor son of a bitch, I hope I never get to that point." I now see how it could happen.

Riess: It's important to establish rapport, but is too much rapport something to guard against?

Foster: Yes, and for a long time I didn't give my Berkeley address to people in Tzintzuntzan, most of them, because I was afraid I'd be inundated with people here. Now they can get it very easily. But it's not really a serious problem. It hasn't been with me.

Riess: I was thinking not so much about inundation by visitors but about becoming less dispassionate, more an ally, less a scientist.

Foster: I do sympathize with the people. I think anthropologists almost always sympathize with the people they study. I don't think they're dispassionate. I think we do identify with the people.

Riess: Can you think of any examples where that got in the way at all?

Foster: I don't think of any cases of my own research. There have been instances--I can't think of them offhand--but there have been instances in which anthropologists have had problems. I had problems when we did our field work with the Popoluca and we thought we were going to settle down in this village, and after three days we realized the people weren't going to have us, and we moved on to the next village, where we hung on by tooth and nail till we did establish rapport.

And in the case of Ihuatzio, six students and two professors dropped down on this village, and after three days we found the same thing. There it was a question of numbers, I think. That was not stupidity but lack of experience. In the first case,

Popoluca, we couldn't have known that the people were just suspicious of all outsiders. The fact that we had a letter from the governor meant nothing because nobody could read. I remember showing it to one mayor, and he held the letter upside down and nodded sagely and gave it back to me. [laughter]

Riess: Suspicion of outsiders could be a subject to study in itself?

Foster: I'm sure there are villages that are much easier than others. It depends on lots of things. Tzintzuntzan now, it's no problem for anyone at all to come into, they've been exposed to anthropologists for over fifty years. And even if they hadn't been exposed to anthropologists, it would still be quite different because the people have been in this country, the USA, many of them, and it's no longer an isolated, closed community.

Predicting and Theorizing, Long-term

Riess: *Long-term Field Research in Social Anthropology* came out twenty years ago. Is that a book that's going to be updated? Has the thinking about long-term field research changed?

Foster: Van Kemper, who was the fourth editor, is putting together a new edition. He'll be the senior editor of the new edition. [laughs] He's doing most of the work. In addition to most of us who are updating our earlier articles, he has brought in a number of other people with new material, so I'll be interested--I have not seen any of the other articles. I think he hopes to have it out in about a year. That will be interesting.

Riess: Will the argument be different or as powerful for the value of long-term field research?

Foster: It will be much more powerful because at that time, in 1975, when we had the Wenner-Gren symposium at Burg-Wartenstein, we had a difficult time getting enough people together who had had ten years of experience in the community to justify the conference. Now you could get five hundred who have twenty years experience, I'm sure. It has become the standard thing.

I think every anthropologist, social anthropologist, assumes that he or she will maintain contact with at least one community over a long period of time. I don't think the book is sufficient explanation, but I think perhaps it justifies it for some people. And I think others just came to the same conclusion that we did earlier, that no matter how hard you work, a single long trip is

not enough, you've got to go back to get additional data. And also, in this period when change is the rule rather than the exception, I think the discovery that change is something that anthropologists should and can study is very basic.

Riess: In this edition there is theoretical justification, but it would not be necessary in a future edition to even spell it out.

Foster: Only as an historical item.

Riess: In the *Long-Term* book you say that anthropologists have been negligent in making predictions to be tested in subsequent visits. How about now?

Foster: I haven't followed much of the work that's been done. I don't know to what extent anthropologists are making predictions when they initially go to the field and then, some years later, check them out. I'm sure that those who have had long-term experience think of things they predicted, or thought would happen, and have evaluated their initial suppositions or hunches. That would be one of the interesting things to work out, I think, for the new editor.

Riess: How much it is after the fact, and how much it is predictive?

Foster: I think now we've learned that anthropologists are not awfully good predictors as to what's going to happen. At least that's the impression I have. I hope I'm wrong. But I just don't know. As I say, I've not followed much of the recent literature.

Riess: Maybe it hampers your powers of observation to be in the process of predicting at the same time.

Foster: I think prediction comes--if you don't formalize it, you have hunches, unconscious and conscious hunches as to what's going to happen. In the case when I was there, in 1945 and '6, I wasn't making predictions as to what would happen. But when I reviewed my studies, I began thinking about what I had thought, and culling my notes for hunches that I had.

What I failed to realize was how rapidly change could and would come. I think that's true of every anthropologist all over the world. I think the postwar changes have been far more rapid than anyone ever suspected. I mean, not just anthropologists but everyone. The world today--the fifty years since the end of World War II I think have changed the world more, almost, than the two hundred years before that.

Riess: In the book, on page 348, you say there are "few examples of major theoretical formulations that have evolved out of it."

Foster: I suspect that's still true, that somehow we haven't latched on to all the implications that must be there. In my case at least--I know Evon Vogt, in his monumental work in Chiapas, feels the same way--the increasing richness of the data and the ability to give depth to the analysis of institutions and activities have been more important than the specific social and cultural predictions about change.

My hypotheses about limited good, for example came from richness of data. My ideas about the origins of humoral medicine in the Americas came because I began delving into the problem of ethno-medicine.

[tape interruption]

Riess: [Referring to two articles Foster gave Riess after the previous interview.] The fallen fontanelle article you gave me had an overwhelming amount of convincing data.¹ But what is the fallen fontanelle?

Foster: That's the soft spot on the head of a baby.

When a child is badly dehydrated the fontanelle sinks, and the child is ill. In Mexico, and other Latin American countries, they believe that the falling of the fontanelle is what causes the symptoms, so there are remedies to try to raise the fontanelle. In that article I was responding to an article by Bernard Ortiz de Montellano. Ortiz de Montellano wrote an article on popular folk medicine in Latin America, saying something that I hadn't even mentioned. I'll get the article. It's a very short one.

[looking through papers] I say that humoral medicine in Latin America, the hot and cold, is European in origin without doubt--it's Greek humoral medicine. But there were some anthropologists--Ortiz de Montellano is the most explicit--who deny it. [gives a reprint to the interviewer] You can take that along if you want to. And this, the two of them have to go together.

It's hard for me to see how anyone could reject the evidence, it is so overwhelming. When people don't want to

¹ "Relationships Between Spanish and Spanish-American Folk Medicine," *Journal of American Folklore*, Vol. 66, No. 261, 1953; "Fallen Fontanelle in the American Southwest: A Commentary," *Medical Anthropology*, Vol. 13, 1992.

believe something, the facts have no bearing on their mental processes, unfortunately. [chuckling]

"Wisdom of the Elders"

Riess: In the *Long-term Field Research* book you also bring up the age factor. That is worth thinking about, the fact that anthropologists start out young and unlined and naive, in a way, and they change.

Foster: I'm more impressed by that now than when I wrote that section of that article in 1975. I told you that in 1978 Mickie and I dropped in on Alfonso Villa Rojas, who's one of the authors in this book. He was in Chan Kom, ostensibly doing field work--a young woman student was helping him. He took us around the village, and I thought, "My gosh, Alfonso really is not in touch with this community anymore. How can that be?"

I realize now, after more than fifty years myself, I don't know much about Tzintzuntzan, actually. My knowledge is of the village in the past. Peter and Alison know more about what's going on today than I do. Van Kemper and Stanley certainly do, too. I'm not there enough to keep up. The old people, my friends, have died. The young people I don't know. They may have been in the States for years. So I'm a very poor source of information on the contemporary village.

Riess: That's just a given?

Foster: Yes.

Riess: Even if you stayed there, you would become out of touch in that any older person in any society is out of touch?

Foster: Yes. It means the data I gather are less good than the data the younger people gather because the people I would gather it from are, if not my age mates, they're in their sixties, which seems pretty young now to me, but--.

Riess: What do you conclude from this? That people should hang up their research boots when they're in their fifties or so?

Foster: I think I was having good ideas until my early seventies. I think the last, what I consider to be a major hunch that I think is a good idea, is the "Validating Role of Humoral Medicine" article

that came out in 1988.² I think I was in my mid-seventies when that idea entered my mind. So I'd say until my mid-seventies I was still very productive of fresh ideas. After that I've reworked ideas, but I haven't come up with anything I consider to be of the magnitude of "dyadic contract" or "limited good" or "envy."

Riess: People in their seventies and eighties go on being consultants. Is there something different about being a consultant? Or do they have limited value as consultants?

Foster: I think it depends on the individual. There's just so much difference.

Riess: And older people remain on corporate boards, and to be called upon for maybe a different kind of wisdom.

Foster: I think there's more danger of keeping people too long on boards. My father was always sensitive to this, and I inherited this from him, a feeling that you reach a peak and then it's best not to take part in policy decisions. I think it's a great shame that a university professor can now go on teaching as long as he or she wants to. For every genius you have, that you're delighted to have, you're carrying ten or twenty deadheads whom you wish had retired at the age of sixty.

The wisdom of the elders is something that applies only in a stable society, of which there are none anymore. Otherwise the wisdom of the elders is questionable, except for a very few exceptional people. Society has to play the averages if it's going to be successful. That means losing the wisdom of a few exceptional people perhaps in order to prevent the dead hand of a great many unexceptional people.

Riess: Have you ever talked about that with any of your various collegial groups, like the Little Thinkers?

Foster: I don't recall discussing that exactly, but we do talk about various things.

[laughs] I don't think you can make as much of the Little Thinkers as you'd like to make of it. It is much the most interesting group of people that I belong to, or have ever known, so many different ideas and talents, and the conversations are always lively. I'm overwhelmed at the collective amount of

²"The Validating Role of Humoral Theory in Traditional Spanish-American Therapeutics," *American Ethnologist* 15, pp. 120-135.

knowledge in the heads of those people, it makes my mind swirl when I think of how much information they have at hand, in various fields. But usually I can't remember at the end of the lunch what we talked about, except I know I've been entertained vastly.

Riess: I guess I was hoping for a collective wisdom, like that of the elders of a society.

Foster: I've never thought of it that way.

Riess: Most primitive societies do venerate the elders?

Foster: Primitive societies are stable societies, they're not changing. And one reason they're not changing is the elders have got control of them, and they don't want them to change. That's not the basic reason, but I think that's a factor. The two go hand in hand. They're wise because of the accumulated years of experience in a society that has changed very little. But in a rapidly-changing society, the experiences I had in the 1920s and the 1930s--what bearing do they have on society today? Very little.

You wouldn't want the United States to be run by a gerontocracy of eighty-year-old people, regardless of how well they've done in the past. I know there are exceptional people--George Kennan, I'd say, appears to be one.

[tape interruption]

Foster: I was the token conservative when I joined the Little Thinkers. Now I'm the radical, anti-religion leftist.

Riess: Why haven't you reverted to your conservative roots? Most people do, the older they get.

Foster: I think I'm like my mother, who was an interesting person. She was raised as a fundamentalist in the Methodist Church in Iowa, in the heart of the Bible Belt. My grandmother read the Bible every night because she couldn't sleep. And I grew up unquestioning about religion. She, my mother, as she grew older, became more and more open. Before she died, she concluded there was no real hereafter, which I thought was remarkable. She was ninety-five when she died. She didn't expect to see my father. She hoped she would, but she didn't expect to see him. I think I have some of her line of thinking.

Riess: Such a good role model.

Foster: Her mother was her role mother. My grandmother. I remember her very well. She lived until I was about a sophomore in college.

Mama said that her mother always told her, "Be prepared to go more than halfway, meeting people." I have often thought that if I am successful in establishing rapport with people in Mexico and other places, that's one reason, and I very much bear it in mind. Mama always was sensitive to other people's feelings. She realized they might feel very differently than she in a particular situation, and she tried hard to understand how they felt.

My father, who was a very warm and sympathetic person, didn't have that same talent. He did to a degree. But I know Mama wished that he was a little more sensitive on occasions as to how his behavior might strike other people.

Riess: Even to the extent of expectations of his own children?

Foster: I'm not sure I know what you mean.

Riess: You've talked about how the family expectations were that all the sons would come back and have a role in the business. Your mother maybe didn't feel the same way as your father did about that?

Foster: I think she felt exactly the same way.

I was unfair to my father in what I just said. He was often sensitive. The folks built a house they moved into in 1924. It cost, I remember, forty thousand dollars--I don't know why I remember the figure, but I have this mind for trivia. I remember my mother told how my father wrote a letter to the contractor, a man named Bob McMasters, thanking him for the great job he had done in building the house, telling him how much he enjoyed it. After he died, his widow told my father that her husband had saved the letter. So he was sensitive, too.

Modern Methods, Modern Medicine

Riess: In the conclusion to the *Long-term* book you talk about coming to understand yourself and your motivations and inner conflicts to a degree you doubt would have been possible had you spread your research efforts over several projects. I wonder, taking that as a text, whether you ever experienced irritation, in the field, at what would seem to be superstition and basic ignorance.

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Foster: You do indeed feel irritation. I remember we'd suggest to people things we thought would help them live better. We said when they

were beginning to put stands on the highway to sell pottery, "Why don't you ask the government to let you have a plot of land at the crossroads on the outskirts of Pátzcuaro, where all kinds of tourists come? You could have a wonderful store there, and you could sell ten times what you're selling here." They thought that was a good idea, but nobody ever did anything about it.

I don't know whether they were wiser than we were or not, but it's kind of irritating to have been told, however indirectly, that your ideas are no good. We think that if they had done that, they would have had a good deal going. But they were never able to bring themselves to cooperate or to ask to do it. After a while, you learn that--they have a saying in Tzintzuntzan: "Even with whips, you can't persuade people to do things they don't want to do--*aun con chicotes no se puede*." In other words, you can drive a horse to water, but you can't make him drink!

Anthropologists don't go into the field really expecting to change the people. In fact, many of us--all of us were trained not to try to change them. Only later have some of us thought if we had an idea that would make things better for them, it was worth passing the idea along, at least. But we learned that they're going to change when they feel like it. Their wisdom may be greater than ours.

I'd say that's the area in which in international health I've changed most. I started out with the assumption that naturally Western medicine and the techniques of delivering Western medicine were for the benefit of everyone. I think most of us at that time felt that way. We've come to believe that that's not true anymore, that there's more relativity involved, that there are reasons why people practice certain forms and perhaps their forms are better. I think the growth of alternative medical forms in this country is quite phenomenal.

Riess: That must be interesting to you. Does it validate a lot of what you have learned?

Foster: I'm not very sympathetic to it, actually. I think most of us who grew up in a pre-penicillin age aren't. When you grew up in a period in which the doctor literally could do nothing except watch for the crisis, and say to the parents, "I think he's going to make it"--I think I told you how when I was about twelve I had an infected right index finger. They didn't know whether they were going to have to amputate the arm or not. I soaked it in hot water every hour and carried it around in a sling until, fortunately, I got on top of it. A shot of penicillin would have cleared it up in no time today. No amount of alternative medicine can compete with penicillin.

And in 1936 I almost died of a strep throat. In the fall of that year, one of the sons of Franklin Delano Roosevelt had exactly the same thing, and in the Massachusetts General Hospital he was cleared up in a few days with a marvelous new drug called sulfa, sulfanilamide.

Riess: How were you treated?

Foster: I wasn't treated with anything. They gave me a diphtheria shot. They said they didn't think it would do any good, but diphtheria is in the throat, and my throat was awful sore, so they said, "Well, let's give him a diphtheria shot." But it had no effect. And I wasn't even in the hospital. The hospital wasn't air-conditioned, and our home was air-conditioned, and I was better off at home.

I, as you see, managed to squeak by. But I've often thought that people who are so critical of contemporary medicine don't know what it was like before these things came along. The things that killed people then were quite different from what kill us now. Infectious diseases--we had no control over them at all, and basic infections. Blood poisoning was something everyone feared. The son of Calvin Coolidge died because of a blister on his heel that became infected after playing tennis. Twenty years later, a shot of penicillin would have saved him.

Reflections on Privilege, Family

Riess: Another subject. You come from upper middle class America. I wonder whether anthropology started out that way, anthropologists coming from an elite class. And did that start to change at some time that you could put your finger on?

Foster: I never thought of that, but I suppose--certainly until the First World War, anyone who was in academic life, most people in academic life were from the comfortably well-off classes, if not the elite classes. That was certainly true at Harvard and I would guess at other schools, too. The mere fact that you were able to go to college and have a university education meant that you were fortunate beyond the means of most people.

Certainly, when I was a graduate student that was no longer the case. I was in a privileged position in that I didn't have to worry about where the money was coming from for tuition. Or I could get married without having to worry about not having enough

money to keep a wife going, or having to have her work to keep me in college.

But certainly--and I've been thinking a lot about this since I've started talking with you, wondering how some of the people in the oral history group who have grown up with enough money so that they didn't have to worry about whether they could go to college or not, how their accounts go. How do they handle this matter? Are they reluctant to talk about money and what it meant?

[tape interruption]

Riess: The question is, how did being comfortably well off affect your life?

Foster: My grandfather, T.D. Foster, was the first person in the family who had a good deal of money. When I say "a good deal of money," I don't know how much he had, but he was certainly the wealthiest person in Ottumwa, Iowa. My father says he married the boss's daughter. He married one of the Morrells.³ The John Morrell Company had been started, according to the family history, in 1827 when somebody in Bradford bought a barge-load of oranges on speculation, and sold it and made a profit, and decided that produce was a good business to be in.

As to how they got started in the meat business, I don't know, but the founder was named George Morrell. John Morrell was his son, after whom the company finally took its name, John Morrell and Company. There were Morrells in Britain in the company until it was sold out of the family about 1965. But long before I was born, my grandfather, T.D. Foster, became the dominant person in the company during the later years of the 19th century, when he established the business in Ottumwa, Iowa in 1877, and in 1910, with his three oldest sons in the business, made the decision to build a second major packing plant in Sioux Falls, South Dakota. Until the year 1927 it was a closed corporation. That is, it was not open to the public. All the stock was held by members of the family.

My grandfather, whom I don't remember, although I was two years old when he died--I came to understand him after reading Max Weber [The Protestant Ethic and the Spirit of Capitalism] and [R. H.] Tawney on Religion and the Rise of Capitalism. I never understood my family until I read those books in college. My father saw him as a firm, overly demanding parent. I think I told you how my father took me to see "Life with Father" and said,

³See earlier narrative of Foster's family history, p. 1.

"That's what life was like in my family." What he didn't tell me until quite late was a very interesting story.

As I have just said, my grandfather established the packing house in Ottumwa in 1877, and it did well right from the beginning. He used to go to Europe every year to report back to the headquarters in Liverpool. One bit of information I remember is that he crossed the Atlantic fifty-three times, which was a lot of traveling. And he always went first class, obviously.

About 1890 he decided--he had a big family, ten children by two wives altogether. (Not simultaneously). And about 1890 he started to build this big mansion, the nicest home that had ever been built in Ottumwa. About halfway through the building, the packing house was destroyed by fire, so work stopped on the house for about a year. I had always thought that it was because all of the available people were engaged in rebuilding the packing plant. But my father said, "No. He felt the Lord was telling him, 'You've got too much hubris. You're getting too big for your britches.'" In other words, he was afraid for a year to continue the building.

He certainly thought that his economic well-being indicated that--and it fit perfectly, of course, with his Presbyterian religion, and as Protestantism was described by Weber--he was one of the elect. It's a philosophy, of course, that fits beautifully with people who make money and are religious. The fact that you make money indicates that the Lord approves of what you're doing.

So it was the most powerful family in town, if you can speak of a "powerful family" in a town of 25,000 people. My father grew up under the conditions I did, that is, he didn't have to worry about whether there was going to be money to eat or buy clothing.

Riess: You told me much of this earlier, and so I don't want you to repeat--I mean, I want to get to your part of the story.

Foster: Well, when my mother and father were married in 1912 he was living on his salary, his father was not giving him any money. My folks lived in a small cottage, in which I was born, so there was no money--not a great deal of money around then. In Sioux Falls, where I lived until I was nine, I was not really aware of the fact there was any difference in our way of living from any of my schoolmates. We lived in a house that my folks had to some extent rebuilt, but it was just like the other houses on the block. There were other families much better off.

But coming back to Ottumwa when I was nine, I began to realize that the Fosters had money that not everyone had. I

accepted it, I just assumed that was the way life was. But in retrospect, I began realizing that I didn't have to worry about being cold in winter, I didn't have to worry about having medical care, that the thing that determined whether money was to be spent was not did we have it, but was it morally appropriate, was it best for the person.

We were taught that just because we didn't have to worry about money didn't mean we were better than anyone else. I think our allowances were smaller than any of our friends. That's very good, but it also had a negative consequence, I think, in that it made us doubt our ability. I never had any self-confidence, and my siblings didn't, either. I saw my father as the powerful businessman who always knew exactly what to do, and I'd never know what to do. Fortunately, along the way I began realizing, by the time I was in the university, that I could pretend that I knew what to do, and I could often get away with it.

Mickie, when she met me, thought I was very self-confident, she told me. If she only knew! I remember when I was about thirty-five seeing my father in some situation--I can't remember what it was--but it dawned on me, "My God, he doesn't know what to do." It was a great shock. I just couldn't imagine him ever not knowing exactly what to do in any situation, because I was always wondering, in every situation in which I found myself, what I should do.

When I went to the university--I did well in high school. I had a ninety-one or -two point average, an A or A-minus. It never occurred to me that I wouldn't get into any university that I wanted to go to. I applied to both Harvard and Swarthmore, and I was accepted to both. I picked Harvard because that's where more of my friends were going.

My catastrophe [engineering coursework] at Northwestern I've told about. But even when I came to Berkeley and began graduate work and realized I was going to get a doctorate, it never occurred to me that I would be anywhere except in a small college, maybe a place like Pomona or something like that, where I'd teach what I had learned at graduate school. I assumed I'd have to have my own library. And no one was more surprised than I was when I began showing signs of being a better anthropologist than I'd expected to be. I was surprised.

Now, I was not really embarrassed by money in Ottumwa, because there were other families that could pretty much do what we did, and we didn't live an exaggerated life. It was not like life on the North Shore of Chicago. We had a maid and a gardener, but no butler or anything like that. Mama did work around the

house, and she loved the garden, she did a lot of gardening. But I had no chores. I never had to do the dishes because we had a maid who did the dishes. I never had to make a bed. So in that way--I never had to wash my clothing. That was all done for me.

We traveled a good deal, by rail initially, and we always went in a Pullman car. It never occurred to any of us that we would sit up all night in a day coach. So it was a comfortable easy life, and I accepted it unquestioningly.

Riess: This theme of not knowing "what to do." When did you start to feel that you knew what to do?

Foster: I don't have a great deal of self-confidence to this day, but I have more than I had as a young man.

Riess: Would you say that would explain the need to be somewhat formal in the classroom situation and wear a suit and tie, the trappings?

Foster: I'm not good at self-analysis.

Riess: Okay.

That's a lovely statement, that you turned out to be a better anthropologist than you expected to be. Can you elaborate on when you knew that was happening? A moment?

Foster: There was no moment of revelation. I guess the first--I wrote a paper called "What is Folk Culture?" that was published in 1953, after I came back from Spain. It was published in *The American Anthropologist*. It was quoted a great deal, and favorably, and it was reprinted in the Bobbs-Merrill series of individual publications. I continued publishing articles and getting fair reviews. I realized that I was doing better than I knew, and it was very comforting, I must say, and I was beginning to gain confidence in my ability as an anthropologist.

The "Limited Good" article, I suppose, was the pinnacle of my recognition. For a number of years thereafter it was widely quoted--not always favorably, but widely quoted. That's what got me into the National Academy, I'm sure, because it was quoted by economic historians and other non-anthropologists.

Riess: And you never needed to make money through your work?

Foster: I escaped much of the uncertainty that many people have. I've been able to take chances--. Not having to worry about having to support a wife and child on my earnings has had, I'm sure, a profound effect on my personality and on the way I view life. For

example, Mickie and I were married in January of 1938, and we took the year off and went to Europe and did in a sense the eighteenth-century grand tour--ostensibly to study French and German for me. We learned a great deal. It was my first trip to Europe. By chance we were there when Hitler took over. It was an exciting and interesting year. But that's something very few students could do.

And when I did my field work in Mexico, there was no question about having to get a grant. We just went down and did our field work.

Riess: How did your family arrange things so that you didn't have to ask for money?

Foster: My father set up a trust in 1932 for his four children. Mine, and I assume the others, was for about two hundred thousand dollars at the First National Bank in Chicago, where John Morrell and Company did its banking. I began drawing the interest, or the dividends on the principal when I was eighteen, I guess, or maybe twenty-one. When I was twenty-five, I got a third of the principal; when I was thirty, I got the second third; when I was thirty-five, I got the third third. So it was all mine from that point on.

And that's what we've done for our grandkids, something like it, so they don't have to ask for money. It's a question as to how much they should be left. Jeremy, and Melissa to some extent, were somewhat handicapped by having money. Jeremy particularly felt guilty about money. I don't know why I never felt guilty. I was just glad it was there. Enjoyed it. I probably wouldn't be in Berkeley today if I hadn't had money.

At the end of 1952 when the Institute of Social Anthropology closed down, we were trying to decide what to do. I could have gone with the Institute of Inter-American Affairs on direct hire and started a big program of applied anthropology in government. I've often wondered what it would have been like if I had done that. But I liked the idea of academic life, and at the age of thirty-nine, I got to the point where I had to get back into academic life or I never would.

So when Gifford--Giff, as we called him--here in the department, wrote me and asked if I'd like to come for a semester to Berkeley, I jumped at the chance. The salary didn't begin to pay our expenses moving out and settling in, but we were able to take a chance on it. We got out here, and we were anxious to stay. I remember I went over to San Francisco and talked with Hal Halversen, whom I had known on the IIAA project, about the

possibility of a job in the California Public Health Department. He would have liked to hire me, he said, but there was no opening.

We debated buying a winery, which I'm glad we didn't do. And then, before I had to take action, the department found funds to keep me as a visitor the second year, with the idea of firming up the arrangement for the tie-in with the School of Public Health.

Riess: You debated buying a winery? To have a second home, or to be in the wine business?

Foster: In the wine business. Anything to stay in California. I'm glad we didn't. I'm glad the public health thing fell through, because the department of anthropology was just made for me. It was just ideal all the way through. The one job I wanted and one job I was lucky enough to get. It gave me the opportunity, I think, to do the best I could have done. I was one of the lucky ones.

Riess: But about the winery. You couldn't have just stopped being an anthropologist and started being a farmer, could you? What were you thinking of?

Foster: I probably couldn't. It was probably a pipe dream.

We were up on the edge of a winery on the west side of Napa Valley during the grape harvesting season, and we saw there was a small winery for sale. We thought, Gee, suppose we were to buy this and go into the wine business. It was a pipe dream. We never seriously considered it.

Riess: You talk a lot about luck.

Foster: I've had luck. Luck has been the dominant theme of my life.

Going back to the question of money and how it affects one. Some of my colleagues became interested in socialism; others went to Communist Party meetings to find out how that organization proposed to solve the economic problems many of them were suffering from during the Depression. In my family there was no suffering, and the Depression made no difference in our level of living. We didn't have to cut back on anything.

I think that made me a more bland personality than I would have been if I had had--I felt I was never really tested. I've never felt I've really been tested until now, when I've got this question of Parkinson's. And when a man is eighty-five before he's thoroughly tested, is he luckier than most people are?

Menus and Timetables and Other Stuff ##

Foster: As I say, I think I have been a blander personality than I might have been if I had had more challenges as a young man.

Riess: How did you choose that word "bland"?

Foster: Just thinking back about my life history. I had many opportunities. For example, I have taken photographs all of my life, but I never developed film or anything like that. I fish, but I never developed great skill in fishing. I went skiing, but I never became a top skier. I used to like pack train camping, but I never learned how to pack a horse. I've never become skilled or proficient in anything. I see that as an important defect.

Riess: You're not competitive?

Foster: I thought I wasn't competitive. But I underwent psychoanalysis for several years. The revelation to me there was that I was very envious of people, that I was fairly competitive, and I had denied it to myself. That was very interesting. And that's what led to one of my best articles, "The Anatomy of Envy," which is based on my own revelation.

But I don't know why I didn't--in many ways, in sports, I didn't have the coordination necessary for sports. The time and effort that most people put in to memorizing baseball averages I spent on steamships, railways, and airplanes. I mean, the history and the facts and the data. But that was not a very good--that was not a group occupation because there was never anyone I could compare notes with. I was alone in that. I don't know why--I've loved ships, I've loved trains, I've loved airplanes. I've often thought I probably was conceived in a Pullman car. [laughing] I used to subscribe to the *Railway Age*--that was wonderful at the time, in the mid-thirties and early forties, when the American railroads were completely revolutionized. Diesel engines and streamline trains, such a change.

And steamships, I've always been fascinated by them. I remember the name of every ship I've ever been on, except one or two cross-Channel steamers. The *Octorara* in 1917, when I was three years old, the *Octorara* from Duluth to Mackinac. And the *Manitou* from Mackinac to Chicago. I don't remember the names from that time, but I must have picked them up from hearing my folks talk or something. By the time I was ten, I knew a lot about boats. I knew what a 14,000-ton boat was.



On a barge trip on the Canal du Midi, France, 1992.

My first experience on a real ocean-going ship was in 1927 when I went to Porto Rico with my brother, Bob, and my cousin, Marian, to spend the summer with my mother's older sister, Grace. We went on the old *San Lorenzo* and came back on the *Coamo*. Then, let's see, what other times? In 1926 we were overnight on the *Noronic* from Duluth to Port Arthur. I remember looking down onto the deck of the *Octorara*, which was tied up just ahead of us, and remembering I had been on the ship, I remembered being in the lounge up in front, with a little cart or something on the carpeted floor, while the ladies sat in wicker chairs on each side and looked at the kids playing.

I'm always amazed at people who take cruises. I say, "What ship did you go on?" And they say, "I don't remember what it was." I just can't imagine that. On one of our trips, we were on the Columbia River, on the *Seabird*. This was about 1991 or so. There was a fellow cruiser who had taken a lot of cruises, and he had a scrapbook of all his ships. Did I tell you about this?

Riess: No.

Foster: He said, "I was on this boat. She was such-and-such tonnage." I knew he was far off, and I said, "I think you're mistaken." After I challenged him several times--very bad-mannered--he said, "Damn it, I'll go and get my book, and I'll show you you're wrong." He brought it up from his cabin, and his mouth dropped, and he said, "Well, damn it, you're right on all those."

Riess: Great moment! But that was fun to find someone who had the same kind of, at least interest.

Foster: It was nice to be able to confirm my belief that I did know something about the topic.

Riess: And your interests also include trains and planes?

Foster: Yes. I had a marvelous collection of timetables, going back more than a hundred years, to about 1870, which was gathering dust. I finally gave it to the library of Southern Methodist University. They were glad to have it. I've given most of my ship books to the San Diego Maritime Museum, and my collection of airline timetables, and particularly my collection of airline menus, I gave to the transportation library at Northwestern. The collection of airline menus I think maybe is unique. The librarian said they never had seen it, they didn't know anybody collected things like that.

Riess: I've never seen an airline menu, at least one that implied choice!

Foster: I think the earliest one in my collection was about 1952, on an Air France Constellation. Planes were all one class up until that time. When the jets came along, at the end of the '50s, the class system also came into being, and the airlines developed elaborate menus, several pages, with gorgeous big pictures of places they flew. That was really a marvelous collection.

Riess: Why did you hang onto things? What is that instinct?

Foster: I don't know. I guess--look at all these things around here. But I don't know why I collect some things and not others.

Riess: You talk in anthropology about masses of data. Is a hobby in the same way a mass of data?

Foster: No. The data you gather because you think you can use it in your writing. This stuff I never thought I'd use anywhere, and I have not used it--other than the fun I've had in collecting it and then giving it away.

Riess: Are there clubs of fellow boating history lovers, for instance, that you've been part of?

Foster: There is a group, but I've never been an active member. There's the Steamship Historical Society of America that publishes a journal called *Steamboat Bill*, which I learned about only a few years ago, actually. There are chapters around the country, but I have not had anything to do with it.

Revelations of Psychoanalysis

Riess: When did you go into analysis?

Foster: Mickie and I were having friction, and she was ahead of me. I decided, at her urging, that I'd give it a whirl. I must say I learned a lot about myself. It was very useful. It began, I'd say, about '55 or '56, or maybe it was '57 or '58, even. I'd have to look it up. I remember I was in the doctor's office when the word came through that Kennedy had been shot. That's one way I can date it. It must have been a year or so after that that I stopped. I didn't do it steadily. I'd get kind of discouraged and I'd stop. And I felt I never achieved complete transference, but I learned a lot. A good experience.

Riess: It was a Freudian analysis?

Foster: Pretty much, yes.

Riess: Why did you say to me earlier that you're not a great believer in self-analysis?

Foster: I didn't say I'm not a great believer. I said I'm not going to try self-analysis. I like to think a little more about it. It's tempting to try to figure out everything, but--.

Riess: Was your analysis an experience that you kept to yourself?

Foster: I kept it pretty much to myself. It's not something I talked about. I don't think that most of my friends know that I was ever analyzed. And I was never completely analyzed. I don't know what completion is. It seemed to me it was a question of one layer after another peeling off.

I had a very good analyst. The time came when we both agreed that it seemed a good time to stop. I saw him about a year ago, by accident. I was down at the Maris Lab, and this elderly gentleman--who is younger than me, or I thought he was about my age--said, "Hi, George Foster." I looked up, and I couldn't think who it was. He told me, and I saw that he had changed greatly. His hair had become thin and white. He was no longer practicing. He was a very good analyst.

Riess: Was it five days a week?

Foster: No, three days, an hour a day.

Riess: On the couch.

Foster: Yes. Hard work.

Riess: You say you've never done things that are really hard. Maybe that was one of the hardest.

Foster: It was. To bare your innermost soul to a stranger on what seemed like an unfair basis, without reciprocity.

Riess: Yes.

You describe yourself as being perceived by your students as stiff or cold or whatever, as a lecturer. Was that something you were trying to change in your analysis? Were you trying to change some manner of being at all?

Foster: No, I was just trying to learn more about myself and hoping to smooth the relationship with Mickie, which obviously was successful.

National Research Council

Riess: Okay. I have a few smallish questions.

Foster: Fire ahead.

Riess: What is the National Research Council? Somewhere Walter Goldschmidt referred to your deep sense of moral purpose being helpful with issues when you were both representatives to the National Research Council. You were there in 1964 and 1967, '68. What is that? Do you have any idea what is being referred to?

Foster: The National Research Council is a branch of the National Academy of Sciences that has committees to deal with various problems. I was on several committees. I can't think of what one he's thinking of there.

Riess: As a member of the Academy?

Foster: This was before I was a member of the Academy. The members of the committees of the National Research Council are not necessarily members of the Academy at all. They draw much more widely, which is very fortunate. I think the Academy probably tends to be a little conservative and stuffy because most of the people, by the time they're elected are fairly well along. I was elected in '76, when I was sixty-two years old, which I'd say is probably about average, about the average age.

Riess: So the National Research Council--does it review research proposals?

Foster: Well, it has committees. Chuck Wagley I remember was on the Latin-American committee for a number of years--that dealt with the social sciences in Latin America. I was on a committee that dealt with--it was set up after our involvement in Vietnam--on the role of social sciences in the military, and things like that. It was interesting.

Fuzzy Wuzzies?

- Riess: A new subject. I came across the expression the "bongo bongo" in the preface to the *Medical Anthropology* book. I was startled. I was also startled last time, when you referred to the "fuzzy wuzzies." I'd like to know whether these are accepted shorthand expressions, or would they raise someone's hackles?
- Foster: I suppose if you spoke to a New Guinean or an African with fuzzy hair and called him a fuzzy wuzzy, it would raise his hackles. I don't know the origin of the term, but it was popularized by Kipling in a poem a century or more ago. Ever since it has been a term fairly widely used by the public at large for any hypothetical non-Western people whose ways are primitive and different from ours.
- Riess: With the current concern for politically correct speech, do you think you would be able to use that in the anthropology department now?
- Foster: It depends on how you're using it. "Bongo bongo" I think is a term that can still be used. I think a lot of people have used it. It's not original with me, by any means. It just means-- among this group, this is the way it's done, among another group, this is the way things are done. Just a hypothetical name of a primitive group.
- Riess: But it wouldn't add up to a racist label.
- Foster: I don't think it would at all, no. Fuzzy wuzzies would be closer to it, because that was a term that came from British colonies, a deprecating description of any "savage" dark-skinned people from a distant place.

Crisis in Anthropology, View from Spring Hill

- Riess: I told you last time that I wanted to ask about the Spring Hill conference and the crisis in anthropology.⁴ The reason I thought the Spring Hill conference had happened just about yesterday is

⁴*Crisis in Anthropology, View from Spring Hill*, 1980, Garland Publishing, Inc., 1982.

because the book I have out of the library had never been borrowed by anyone.⁵

Foster: [laughter] That justifies what I said, that it was a conference that was quickly forgotten by everyone.

I pulled the book off the shelf last night and I thought, "Too bad. It's very good." I like what I had written a lot. I had forgotten, but I read it, and I thought there's a lot of good history of anthropology here. Berkeley figures very predominantly in this [looking through pages]. Cora Du Bois, David Mandelbaum, Ralph Beals.

Riess: Yes, that was referred to as the Grandfathers Group.

Foster: Yes. I was one of the grandfathers. Stanley Brandes was one of the grandsons. Tom [Thomas] Weaver took his degree here. So we had a lot of our students. Jane Lancaster took her degree here. John Ingham took his degree here--he has done very well at Minnesota. Sherry Washburn.

Riess: Do you remember at the time of the meetings feeling that you were getting your teeth into something?

Foster: No. I don't remember the papers, until I looked at them last night.

It seemed to me to be kind of a Burg Wartenstein on the cheap sort of thing. It didn't have any romance. The Spring Hill Center, oddly enough, had been the home of Mickie's first cousin, John Cowles, out on the edge of Minneapolis. He and his wife had decided they didn't want to live there, so they gave it to some foundation, and they built the additional bedrooms and turned it into the Spring Hill conference center.

I liked my article when I read it. I was kind of surprised. I thought it was a minor article, but I think it's pretty good. I called attention to the fact that the unity of anthropology until about the sixties had been the fact that whether our field was human beings, living, or linguistics or pre-history, we were all interested in origin and development. Anthropology was a historical discipline. And that was a common theme, so all of us, regardless of whether we were physical anthropologists, or

⁵Riess, at the end of the previous interview, says, "When I was in the anthropology library, I borrowed the 1980 proceedings from the Spring Hill Conference titled, *Crisis in Anthropology*--a provocative title." Foster replies, "I want to get the volume and see if I can refresh my mind."

archaeologists, or ethnographers, had this common way of looking at our subject matter: Where did it come from, and how has it changed and developed?

And then, when in the beginning--well, British social anthropology, I suppose, first. Ethnography turned into a social science, and biological anthropology developed more and more biology, and prehistory developed more and more chemical techniques--carbon 14. And we lost this common theme. That, I think, is what lies behind this.

The idea of this conference was dreamed up by Richard Currier, whom I mentioned, my student, who was deflected from anthropology because his dissertation was rejected by Phil Lilienthal. He was at Minnesota briefly. Ad [Adamson] Hoebel, who was long-time chair of anthropology at Minnesota, liked him--they liked each other. Rich, I think, suggested the idea that skipping a generation, the grandfather-grandchild approach, would make an interesting theme. Ad Hoebel, as chair of the department, had the power to bring it about. So that's how it came into being.

Riess: Has there been a continuing self-study by anthropologists?

Foster: Yes, more and more. Every once in a while, someone gives a lecture, a distinguished lecture, or writes an article for one of the journals about Where are we, and what we do have in common? One of the most recent issues of *Anthropology Today* is devoted to the question of, Is the culture concept valid? I think it is, very much so. But some of the more radical anthropologists think that we should throw the term overboard.

Extra-Curricular Reading

Riess: In your "A Half Century of Field Research in Tzintzuntzan" manuscript you say you believe [reading] "anthropologists should read widely and generally and seek out cross-cultural experiences as sources of pertinent ideas." That is my way of asking in what areas you read outside of anthropology over the years?

Foster: History and biography I've enjoyed particularly. And technical accounts of engineering. David McCullough's book on the Panama Canal, for example. Some travel books I've enjoyed. Novels I read from time to time, though not as much as some people. I don't read as much now as I used to. I get tired, I can't read as well.

Riess: It sounds like adventure is part of what you like.

Foster: I like the wonderful series of Patrick O'Brian, I think they're marvelous. In his books it's not the adventure that's interesting as much as it is the interplay of character and the knowledge of science at the beginning of the nineteenth century.

Looking at the Family Album

[Interview 10: March 4, 1999] ##

Riess: What kind of photographic equipment have you used over the years?

Foster: I used an old Eastman bellows camera initially: there were six pictures on a roll and each negative was the size of a postcard. But when I graduated from college in 1935 my folks gave me a Leica as a graduation present. That was my first 35mm. camera, and I used that and other Leicas for a great many years. Then years later I went to a Rollei, even smaller than the Leica. But in recent years I've gotten lazy and I've just used Japanese point and shoot cameras.

Riess: Why did you want a small camera?

Foster: Easy to carry. But in those days, of course, you had to take light readings with a separate instrument. And you had to set the stops and everything. It took a lot more skill than it does today. But I think we got better pictures.

Riess: You used your Leica in your documentation of Tzintzuntzan?

Foster: Yes. And I took a movie of Tzintzuntzan, a 16mm. movie, with an Eastman Magazine Kodak. I think I told about this. It made a very interesting film. I've subsequently put it on video and Thor Anderson, who's a specialist, is now doing a remake of the original with sound.

Riess: Most anthropologists document photographically?

Foster: I think so, yes. If they don't, nobody else is going to. Yes. Although we give no courses in photography or anything like that.

Riess: And the pictures are just filed away?

Foster: They're kept by the individual, yes. Also, the Anthropology Department has a small file, or used to, at least.

Makes me wonder what I'll do with my photos. In the travels Mickie and I have taken we have lots of pictures of ethnographic value, I think. A week ago Wednesday, at the meeting of the emeriti anthropologists, Burton Benedict told about an article he had read in the New Yorker about an Italian anthropologist who lived on the island of Kitawa, which is one of the Trobriands. That's an island Mickie and I have been on, on one of our trips, and I've got photographs of the natives and other things. Just a few. I took photographs to show that there was a Kitawa. The Trobriand islanders are the prettiest little people in the world, I think, they're just gorgeous.

Apparently the pictures are very rare. What will I do with them? I don't know.

Riess: Does the National Geographic act as a clearing house?

Foster: I don't think so. I think the Smithsonian is more likely to be the place. It's a full-time job for a lot of people to file photos. The Tzintzuntzan photos and slides I'll leave with Stanley Brandes and Van Kemper, of course. I expect they'll manage the others as well.

Riess: Today you brought a photo album to our meeting.

Foster: Yes, in this album I mix the personal, family pictures, with the trips. [looking at album] This is a trip we took in Spring 1997 from Dubai, around the Arabian Peninsula, up to Aqaba, and ending in Petra. A very interesting trip. Most of our cruises we have taken by ourselves, but on a number we have been accompanied by friends and family.

Now, here is family. This is our granddaughter Zoë, graduating from Kenyon College. She's the only anthropologist in the family, among the granddaughters. She is beginning graduate work this year at Northwestern. This is my son Jeremy and his wife, Angela [Zoë's parents], and Jeremy's older daughter, Emily.

Two years ago we took a trip around Britain with Angela on this little boat, the *Caledonian Star*, starting out in Edinburgh. Here is the Firth of Forth bridge. Angela had wanted to see that --she's English, and her father had always told her that it is one of the greatest sights in the world. And it is wonderful. [Some of the photographs from other cruises are discussed off the tape.]

I put in here at the end of the album a series of shots that I found I had. This is my father's father, my grandfather, who came to America in 1868.

Riess: Do you see a family resemblance?

Foster: No, other than the bald pate, I don't.

That's my brother Bob, who died about twelve years ago. He was a great person. And this is a picture of my cousin Marian [Walker], the daughter of my mother's next older sister, Grace, who lived to be a hundred and three. Marian died of cancer three or four years ago. She was a great expert on Japan and she was with the US government.

This picture was taken in 1952, the last year we were in Washington. We had a big family reunion in Jamaica, the four kids and their spouses. And our folks [pointing out parents, siblings and spouses.] Mickie's parents came down and joined us on the spur of the moment--a few days after this picture was taken. We had a wonderful time for about ten days. Hotels in those days were simple. They were not air-conditioned, all open to the breeze.

Riess: Here are photographs of "Chokecherry Gulch."

Foster: That is the place that Mickie's parents acquired in 1945 in Calaveras County. This is an old picture that was taken in 1953, and that's the car we had in Washington in which we all drove out. Since 1970 we have had our own vacation home at Chokecherry Gulch with Peter and Julie Jurs. We built a pre-fab, which we call Snag, because it was built on the site where a snag had stood.

These are good shots of my mother, and Gene, who lives in San Diego. She was ninety years old when that picture was taken. She was a beautiful woman, almost to the year that she died--had a very positive outlook on life.

Riess: You are wearing an Indian outfit here?

Foster: Yes, these are red pants I got in France, twenty-five years ago, and this shirt I bought in India.

Riess: And a beret?

Foster: I've worn a beret since 1949 when I first went to Spain. I still wear berets.

That's Gene's son Mark and his wife, Nancy. These are Melissa's three daughters: Christy, who is marrying in April of this year, and Eva, who was just admitted to Yale for graduate work in art history with a fantastic scholarship. They called her up, early, and said they would give her tuition for four years,

plus \$11,000 a year if she would come. And this is the Dutch granddaughter, Klaartje, the daughter of Wijbrandt, Melissa's second husband. And these are Jeremy's two daughters, Emily and Zöe.

That's Tzintzuntzan, from the air, looking down. And this is Lola, the older of the two daughters of Micaela, with Mickie's principal linguistic informant, Florentina.

For the last twelve or fifteen years we've made Christmas cards like these. Mickie always used to do them, and then several years ago she said I better start helping. I decided I'd do it the easy way and just take snapshots and have cards made. This is the series, showing things we've done.

Riess: You were on the Royal Cruise Line, the Golden Odyssey, in 1990.

Foster: Mickie had always wanted to cross the ocean by ship again. She had crossed it a number of times before we were married, and a couple of times after we were married. This was a 10,000-ton 450-passenger cruise boat that was being repositioned from San Juan to the Mediterranean. They had a very good price, and so we signed up for it. On that trip we astounded people at the table where we were sitting by saying this was much the largest cruise boat we had been on. For them it was the smallest they had ever been on! [Foster and Riess look at the individual Christmas cards.]

Riess: Here you are at the dedication of the George and Mary Foster Anthropology Library. With the family.

Foster: This is the last one. This was in Turkey this last fall [1998]. And here are pictures of Robert Rubinstein--he has been Mickie's colleague in writing about war and peace--and his wife Sandra Lane, an anthropologist who took a degree in our medical anthropology program. That was an extraordinary coincidence--all three of us in Petra! And this is their daughter Helen, who is about eight now, a little beauty. Here is an early picture of Van Kemper and his wife Sandra and their son John.

Looking at the Family

Riess: In this final interview we're talking about family, particularly the family of two anthropologists who might have had ideas about how a good family works.

Foster: Yes. We had the good role models of our parents. My parents had been married for fifty-seven years when my father died. Mickie's parents almost made fifty years.

Jeremy came along fifteen months after we were married. He was born in Peralta Hospital in Oakland, a Caesarian. He was ten pounds. [laughs] He was two weeks old when he was born, I used to say. Mickie always laughed because I was kind of embarrassed to ask to see him at the hospital. Being a Caesarian, he was not misshapen. I'd ask for Baby Foster, and this great specimen would be held up, not one of the little wrinkled ones. I was afraid of envy even then, I guess. He was such a prize specimen compared to all the others. Most babies in their first few days are not particularly pretty, except to their parents.

Melissa arrived three years after Jeremy, born in the Syracuse University Hospital. While I was at Syracuse I shared an office with a young sociologist named Charles Bowerman, who was completing his doctorate at the University of Chicago. This was in the fall of 1941, the first professional job for both of us. Charlie's wife, Madeleine, was pregnant, as was Mickie. Madie gave birth to Bill in mid-March, and Melissa came along six weeks later, on April 3, 1942.

Both babies were delivered by the same obstetrician in the same hospital. They didn't know each other until they were graduate students at Harvard. One year--it must have been spring of '65--Mickie and I were on sabbatical in Cuernavaca, and I came up to Boston for a meeting, where I saw Melissa. I asked in my usual thoughtless way, "Do you have any good boyfriends now?" She said, "Well, there's a fellow named Bill Bowerman that I kind of like."

I said, "Is he by any chance Charlie Bowerman's son?" Her eyes popped. She said, "How did you know that?" I said, "I was guessing. The name is unusual enough." And then I told her the story about their common background, which she had never heard. She was astonished. Their friendship flourished, and a couple of years later they were married. We've been friends with the Bowermans ever since Syracuse. They come to Snag in the summer, and we visit them in their home in Tigard, Oregon.

Riess: Both you and Mickie were traveling a lot. Going places with two young children can't have been easy.

Foster: Not in those days. We crossed the country by train, always. We never seemed to go anyplace less than transcontinental in those days. We felt like Polish peasants. We carried diaper pails. In those days, there weren't Pampers or things like that, we just had

to carry a great stack of ordinary, old-fashioned diapers. The diaper pail began getting kind of rank by the time we'd get there.

I remember once--we were going through the Moffat Tunnel on the Rio Grande railroad, west of Denver. The train was pulled by steam engines, and Mickie had just washed Jeremy, and the soot came in through the windows, and he was just as dirty when we got out of the tunnel as he was before she had given him his bath. Yes, we did a lot of traveling.

Riess: What were the child-rearing theories that you subscribed to?

Foster: At that time--that was before Dr. [Benjamin] Spock came along--it was feed by the clock, every four hours. That worked fine for Jeremy, but for Melissa it was the worst thing we could have done. She was always hungry and frustrated. And we were trying to be good parents and trying to stretch out the four hours. Demand feeding would have been far better for her. But she survived very well, I guess that was the main thing.

Riess: There was the issue of letting them cry or not letting them cry.

Foster: Well, Jeremy didn't cry very much. From the day he came home from the hospital--Mickie was in the hospital for ten days, I think, for both kids--Jeremy, you could say, was a month old when he came home from the hospital, and he slept through the night right away.

Melissa, on the other hand--she was a good-sized baby, she weighed almost eight pounds, but she, as I say, would have done better on demand feeding. Mickie was bound and determined to nurse the two babies, and so she did for about four months. That was a great source of satisfaction to her.

We certainly loved those kids, and do.

Riess: As parents, were you students of child rearing theories?

Foster: I don't remember reading any books about child rearing. I don't know whether Mickie did.

Riess: I was thinking about Skinner and Watson, the behaviorists.

Foster: No, I didn't read any of those. We just followed the pediatrician's recommendation, Dr. Hubert Long, who was a well-known pediatrician in those days, in the East Bay. A very nice fellow. He lived to be almost a hundred.

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Foster: Compared to Mexican customs, certainly, and compared to the United States today, I think we were rather rigid with our kids. They had regular bedtimes, and whether they were tired or not, they were always put to bed. In those days it was not customary to take kids to parties, parties were for adults. We'd always get a baby sitter. That was one way young women could make a bit of money, and in Berkeley they were often college girls. Now I don't suppose there's a college girl who wants to be a baby sitter left in the East Bay.

That's certainly one of the great changes. Angela let her kids go to sleep when they felt like it, and get up when they felt like it. I remember Zöe--we'd be sitting around in the living room here, it might be eight or nine o'clock at night, and Zöe would finally get tired and would curl up on the sofa. Angela would just let her lie there until we all went to bed. I must say I think it's much better than our rigid ways. But we had been raised in the same rigid fashion.

I can remember having to take a nap every afternoon in my early years, lying there, waiting for it to be time to get up, hating every minute of it. I like the contemporary system much better than our system, although I think some parents are overly easy on their kids. I think they don't think about the way they may be annoying other people.

Riess: I remember once you said that the kids were sometimes left with grandparents when you and Mickie were travelling.

Foster: We were lucky. We had parents who were willing to take them, and who had the means to take them, who had enough help so they didn't have to do all the work themselves. In the winter of 1940 we left Jeremy with Mickie's parents in Washington for about six weeks while we were in Mexico studying Spanish. Mickie's mother had hired a full-time nursemaid for Jeremy, I think. They got along fine.

The next year, when I did my dissertation work in Mexico, Jeremy stayed with my parents. By then he must have been about two. He quickly forgot his parents, I think, in both cases. Though I remember one of Mama's stories was that after we got back, and we were in Ottumwa, and I was still in bed one morning, Jeremy said something about, "I've got another Papa upstairs." [chuckling] I forget how the story went, but it was very amusing. He was kind of confused at the idea of having two older men in the house--my father and me.

That was hard on Mickie, though. She said she'd not do it again, though I don't think Jeremy suffered. Maybe he did.

Riess: How about school choices? Did you have ideas of what kind of schooling was desirable?

Foster: We assumed they'd go to public schools. Jeremy went to nursery school in Syracuse, and Berkeley, and also the year when we were at UCLA, 1942-43, following Syracuse. I don't think he'd started first grade when we went to Mexico, where the school situation was quite different. We put him in a school there run by Mexicans, which had a number of English-speaking students as well. He did very well there, he liked it. Melissa was always very resistant to that kind of schooling, but at that time she was only two, so it wasn't a problem.

We were lucky in that in Mexico we had good doctors. There was a British-American hospital--there was then and still is. And there was an excellent pediatrician, Dr. Julia Baker, the American wife of one of the agricultural attaches at the embassy. There was an old-line American doctor, Dr. Garnett, who had practiced in Mexico most of his life, who was a very good doctor, too, we thought.

We had always said that if we had something serious we'd get back to the States as fast as we could. One night I remember Jeremy had a terrible stomach ache. We called Dr. Baker the next day, and she came and said, "I think it's appendicitis." We went to the British-American hospital, and before we could say "boo" Dr. Garnett had taken out his appendix. Jeremy came through fine. That gave us more confidence.

We were a little surprised--well, he was astonished when they gave him *cafe con leche*. He said, "Mama, I'm just a little boy. I'm not supposed to drink coffee."

Riess: [laughs] They grew up multilingual.

Foster: Yes. Jeremy speaks excellent Spanish.

Melissa, at the age of four when she came back, spoke the Spanish of four-year-olds. I think I've told this story about how we stayed with Mickie's parents for a week or two when we came back in the summer of '46. Alma Garrett--later head housekeeper at University House for many years--was at that time the cook in Mickie's parent's home on Benvenue. Melissa knew that she spoke Spanish when she was in the kitchen, and English when she was in the living room--so she'd go in to get a glass of water from Alma and ask for water in Spanish, and Alma was much amused.

We tried to keep the Spanish going. When we got to Washington, we found a Cuban maid, Maria, who spoke not the good Spanish we liked but good enough. But we couldn't get the kids to

keep it up. Once they were playing with Americans they were ashamed to try to speak Spanish.

In Washington we tried putting the kids in public schools, but the schools were not very good at that time. So Jeremy went to the Georgetown Day School. It had been started by an anthropologist named Philleo Nash and his wife. Philleo had lived in Washington and worked as a government anthropologist for a good many years. I think he had been a lieutenant governor of the state of Wisconsin at one time, too.

Riess: A school started by an anthropologist. Did he have some theories that made it an unusual school?

Foster: The unusual school was the one Melissa went to, which is called Greenacres, on the board of which Mickie served for the years we were there. If you do her oral history, which I hope you will, she'll tell you about that. It was a good school then--very small, just getting started. Open classrooms, and close relationship between the students and the teachers, and the teachers and the parents. That was an excellent school. And I think the Georgetown Day School was excellent, too. So they had very good education there. Later Jeremy went to the Landon School and Melissa to the Potomac School.

When we came to Berkeley they went into public schools. And there were, at that time, still sound schools here, including high school. There weren't the race problems then that there were later.

Riess: This was before busing.

Foster: Yes. But Berkeley High School was thoroughly integrated. Melissa and Jeremy both talked about how they had friends who were from other races, and there was none of this fear of going into the bathroom or things like that. It's just hard to believe how quickly that came. Jeremy graduated in '57, and Melissa three years later--well before the high school went all to pot.

They still had tracking at that time, although it was not exactly tracking, it was more that students and parents chose the curriculum they preferred. I must say I think that's a good thing, judging by what we've seen in Holland, where they don't hesitate to track students. It means the bright students aren't sitting around twiddling their thumbs while the less gifted students are learning something. It pushes them more to their potential.

Melissa had wonderful Latin courses in high school. And when we were in Rome in '61 she delighted in reading all the inscriptions on the stones, which she was able to do.

There was a period there, in the years after we came to Berkeley, when I was not on good terms with either of my kids. I was a very rigid parent and tried to push them. And I don't know why. Melissa certainly was the last person on earth who needed pushing.

I think some of my--when I was a kid at home growing up I was always expected at dinnertime to wear a jacket, and even a tie, and I thought it was not unreasonable to ask my kids to at least wear shoes to dinner. If I had had a little more support from Mickie, I think it would have been better, but we argued about it. I was unquestionably rigid and demanding. I thought what I was asking for was entirely reasonable. Subsequently I realized that times had changed.

Riess: Who won, as it were?

Foster: I don't think anybody won. I don't think that's the right model. We all lost. I certainly lost.

Jeremy

Foster: Jeremy is as bright a person as I know, he's got a first-rate mind. But he suffered from--Jim [historian and dean James] King used to call it "professor's son syndrome." Jeremy thought I was a real brainy professor, that he could never live up to my model, the same way I thought of my father as being a successful businessman, that I could never hope to imitate. I was lucky in that I got into a different field from that of my father, where I could be on my own and not be judged against him.

Jeremy--in high school he got by, but he didn't get very good grades. Previously he had had excellent grades. He wasn't able to get into Cal or Stanford, but he didn't want to, so he went to the University of Colorado in Boulder, dropping out at the end of his sophomore year. Then he joined the Army Reserve, and after boot camp at Ford Ord spent six months on active duty and going to camp each of the following six summers. He then reentered the University of Colorado at Boulder and graduated two years later.

After graduation he spent six months traveling in South America, where he perfected his Spanish. Jeremy has his mother's knack of catching accents, and he was much amused--we were too when later he told us about it--when in the course of going from one country to another the local people recognized that he was not a native of their country, but assumed he was a native of the country he had just left!

When he returned from this trip he entered Thunderbird Academy in Arizona, and took a second Bachelor's degree, in "Foreign Trade," graduating as valedictorian of his class. After this he spent a couple of years in graduate school at Columbia, studying linguistics, including Chinese. It was at this point in his life, I think, that he realized he was not interested in the academic life.

Riess: What did he do with his business degree?

Foster: It got him a very good wife! He joined a Colorado friend in Freeport in the Bahamas, and they built and operated a very successful fried chicken restaurant which they called "*The Boss Bird*." He enjoyed that. He enjoyed the challenge of finding out about cooking equipment. It was a good little restaurant--we were there once about 1968.

While he was there he met Angela, who had come to teach school in the colonies, and they fell in love and were married. Then with the Bahamas independence, business became very much more difficult for Americans, and they thought there were better places than the Bahamas to raise kids. So they moved to Aspen, and there Jeremy developed remarkable skills with woodworking.

My father was very good with his hands, with tools, and Jeremy must have taken after him. I think I showed you a couple of pieces in there, in the living room, that Jeremy made. "A chip off the old block." He made some special doors for John Denver, I remember. He also built his and Angela's home. And on one occasion he took over a house that was about half built and finished it and sold it at a nice profit.

He dealt for a number of years in fine woods. He'd buy a batch of walnut logs in, say, Wisconsin, and sell those he didn't use himself. He imported a good deal of fine hard wood from the East and Midwest, always selling what he didn't need for his own work.

So he has led a different kind of life from most of the other men in his family. He values freedom and independence greatly. He has been an awfully good father and a good husband,

and he's much appreciated by all of his friends and colleagues in Colorado. He's a very talented person. When they want a job done, they ask Jeremy. He "runs" the Colorado River, white water rafting, on occasion, whenever he can get a permit. With colleagues, but he's the one that runs the show, organizes it.

Riess: Do you think of Jeremy's as a sixties story? How much do you think the times account for Jeremy's choices?

Foster: I've often wondered. I think about it often. If I had realized how conditions of life had changed, I would have been less harsh on him. I think part of the problem was--I don't think the fault was entirely mine. I think that as a teenager he was perhaps more rebellious than many kids. For instance, my father had always insisted that I keep detailed records of my money, and he taught me bookkeeping. And I tried to teach Jeremy, and he fought it every inch of the way.

By then, of course, neither he nor I needed to be bookkeepers. The banks provided their clients with all the information one needed for tax purposes, you didn't have to keep the kinds of records my father had felt necessary when I was Jeremy's age. But I was slow in realizing that. I criticized, chided Jeremy for that. He'd get mad.

Those were really difficult years for both Jeremy and me. But the important point is that we came through those years, and now we feel very close to each other.

Riess: How did you change during that time?

Foster: Well, I eased up. Oddly enough, one of the most important events bringing us together had to do with money. He was married and living in Colorado at the time. He called me on the telephone and said, "Pop, I'm having some financial problems. Can you loan me some money?" I was pleased that he had come to me, and I loaned him what he needed. I didn't ask for interest or anything. He got on his feet again and paid back the whole debt, with interest. I think that was one of the experiences that brought us together. And we've both grown older and more mature and have come to see we really love each other. It's a pleasure to be with him.

Melissa

Foster: Melissa, we didn't worry about her. She loved the orchestra in high school; she played the flute. She took a course at Cal her

senior year and had no problem getting into Stanford. She was always tops in studies. She expected to go into anthropology--her undergraduate degree was psychology--when she went to Harvard. But she didn't like anthropology as much as she thought she would.

Then when she learned about psycholinguistics and Roger Brown, and Soc Rel--as Social Relations was called at Harvard--she, like her mother and me discovering anthropology, she just fell in love with it and was a natural for the field, and was one of the early graduates in this area. So she started with the kind of advantages I had when I was first an anthropologist, being a member of a very small, select group of people.

Riess: Where has it taken her?

Foster: She and her husband--her first husband, Bill--went to the University of Kansas right out of Harvard. Bill's a very bright guy, interested in everything, so much so that he sometimes found it difficult to keep his eye on the academic ball. He didn't get tenure, and their style--their personalities turned out to be more different than we'd thought, and there was tension, and they finally, wisely, faced up to it and divorced. The way I put it, there are now four happy people where previously there had been two unhappy people. Their second marriages are in both cases very solid.

Bill has continued to live in Lawrence, and Christy and Eva have gone there regularly to visit their dad and his second wife, Carolyn. So they feel a tie to Kansas as home--Christy not so much now that she is married and expects to live in Holland, but Eva still a good deal.

Riess: What has Melissa done professionally?

Foster: She's a psycholinguist specializing in language acquisition.

In 1979, several years after completing her graduate studies, she was invited to be a fellow for a year at NIAS--the Netherlands Institute for Advanced Study, in Wassenaar--which is sort of like the Center for Advanced Study in Behavioral Sciences at Stanford. NIAS also provided an office for Bill. Among their colleagues were Wijbrandt van Schuur, whose wife, Marianne, was dying of cancer, and Carolyn, a Scot whose husband had recently died.

Two years later they returned for another year at NIAS. By then Melissa and Bill realized their marriage was not working out, and they decided to divorce. After her first year at NIAS Melissa had had a standing invitation to join the Max Planck Institute of

Psycholinguistics at Nijmegen, Holland. To make a long story short, after their divorce Bill married Carolyn and Melissa married Wijbrandt, whose wife had died, leaving him with three-year-old Klaartje, a most welcome addition to the four granddaughters we already had. Melissa took up the job at Max Planck, and Wijbrandt continued his work as a political scientist at the University of Groningen.

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Foster: It's a wonderful job. She's sort of like a perpetual post-doc, in that she can do almost anything she wants, and she's got full support to come to this country to any meeting she wants to attend. It's like an endowed chair, you might say.

Roger Brown was the first psycholinguist who really trained a group of competent people. Melissa was one of the first. Language acquisition was what he was particularly interested in, the psychological processes by which we acquire language. All the theories to date at that time had been done on Indo-European languages, so Melissa said, "I think I'll do a non-Indo-European language."

Because she had Finnish friends at Harvard, with babies who were learning to speak Finnish as their first language, she studied Finnish and followed those kids around with a tape recorder gathering data for her dissertation, which was published by the Cambridge University Press. Since it was the first study of this type based on a non-Indo-European language it quickly was recognized as a blockbuster in the field of psycholinguistics. So she became well known right from the word go.

She comes to this country a half a dozen times a year, though not always to Berkeley. But she's out here about four times a year with us. And she and her husband like to come and go up to the country. As I said, they spend the month of August there--they have for a number of years. Our fax and e-mail bill goes up considerably, but it's well worth it.

Riess: Children are complicating, aren't they?

Foster: They're complicating, but you wouldn't want it otherwise. There was a time when Melissa resented my wanting her to wear shoes to dinner--there was a good deal of friction in our relationship at that time, and later, too. When we really became close friends was at the time of her divorce, when she asked for advice on how she should handle her estate. She called from Lawrence on the telephone and said, "I know you've been critical of Bill and the

way he's handled my money. If you don't want to have anything to do with my problem now, I'll understand."

I replied. "Well, if there's a time when a father can be of help to a daughter, this is the time." She came out to Berkeley and we worked things out together. That event started us on the way to the solid and loving relationship we've enjoyed ever since.

Using Money Well

Riess: As you describe yourself, you didn't need to think about money, and yet you're very careful about it?

Foster: Money is an odd thing. It destroys many lives, and it saves many lives. It's certainly been a strong theme in my own lifetime, and Mickie's, too. We've never had to worry about money, we've always known there's money there. And the kids knew that. I've been financially independent since I was in college, thanks to my father. And Mickie has been essentially the same, thanks to her grandparents, Gardner Cowles and his wife Florence.

In my case, it made me feel like I had to work twice as hard as anyone else to prove that just because I have money, I'm not going to coast through. I was aware of the fact that there were differences, but I must say I don't think I was crippled by it. I was glad to have it, it gave me an independence that I wouldn't otherwise have had. Our kids have felt differently.

Melissa has difficulties in accepting her financial independence; it embarrasses her and she doesn't like to think about it. Sometimes I wish she would pay more attention to these matters, but I have come to realize that everyone has to decide for her/himself, and that there are no universal "right" answers. I have great respect for Melissa's views, particularly the fact that she and Wijbrandt live simply in Holland, aware that life is easier if they avoid what some might consider to be ostentation; they refuse to stand out from their colleagues in life style.

On the other hand Jeremy, while living a simple life, gives a lot of thought as to when, and in what amounts, he and Angela should pass money on to their daughters, and the charitable organizations to which they contribute. I admire Jeremy for the maturity he has developed with respect to handling money.

Riess: You have been careful about your resources.

Foster: Oh, yes. And we've had very good luck, too. Mickie's family businesses, the newspapers [the *Des Moines Register and Tribune*, and the *Minneapolis Star and Tribune*], were sold out of the family in recent years and brought a good deal of money into our joint accounts.

I've had an investment account in the First National Bank of Chicago [since 9/13/99 "Bank One"] for nearly seventy years. From about 1980 to 1997 a really brilliant man was in charge of our accounts. He foresaw the bull market and recommended that we invest in things like Coca-Cola, Intel, Hewlett-Packard, and the multi-national drug companies--advice that we followed, so that by last summer [1998], when I decided it was time to get out of the stock market, every dollar we had invested was worth nine dollars.

What we've been doing in the last year or so is making plans, and passing money on to universities and other organizations we feel are doing important things. We've given a good deal of money to Cal, and not as much, but a fair amount, to Northwestern. It ain't easy to give away money, I've found. It takes a tremendous amount of time and thought--discussions with lawyers and others.

Riess: I know at Berkeley you've endowed the anthropology library.

Foster: The anthropology library was funny--not funny, but odd. We learned that the library was to be closed, and that struck us as being devastating for the whole department, as it would have been. So we got in touch with Meg [Margaret W.] Conkey and Pat [Patrick V.] Kirch, who were not co-chairs, but one of them was chair and the other was about to become chair. I remember we had dinner with them once, and they said that they didn't know what to do.

Mickie and I said, "Well, perhaps we've got enough money that the university will consider keeping the anthropology library open if we endow it." So we ended up giving four hundred thousand dollars--not to the university library, because we didn't have confidence in the university librarian, but we left it to the department, with the proviso that all the income would go to support the anthropology library. This was done so that in case they ever decided to close the anthropology library, the university library would lose that endowment.

We hadn't thought of anything other than saving the department and the library. We were puzzled when a year later Stanley said, "I want you to be sure to come to the Christmas party." And Nancy Scheper-Hughes said, "Now, be sure you come to the Christmas party." They announced there that the library had been renamed after Mickie and me.

I'm sure many people think that we gave the money, sort of buying our names on the library, but that's absolutely untrue. We were absolutely dumbfounded when we found they named the library after us, and embarrassed as well because John Rowe--he's the one the library should have been named after. He's the one that established it and carried it through.

Riess: Now there is a John Rowe endowed librarianship.

Foster: And I think he feels better now. We've always been good friends, and it was embarrassing for us.

Riess: What other causes have you and Mickie supported?

Foster: Mickie, of course, the Ploughshares Fund. She's a member of the board. That's anti-nuclear war, and that's how she became interested in conflict resolution. Next to the university, that receives the most money. We support Save San Francisco Bay and Planned Parenthood. Mickie has also given quite a bit of money to the International Peace Academy, and somewhat less to the local California Tomorrow, and to environmental non-profits.

And there's a small group down in Oakland that has done very well. It started out with a grant from the Ploughshares Fund that Mickie recommended. It's called the Pacific Institute for SIDES [Studies in Environment, Development, and Security]. They're doing very good work on water in California.

Current Thinking and Writing

Riess: What are you writing now?

Foster: The most recent thing I've done is a paper that I've mentioned before, with a young Mexican historian, Luz María Hernández-Sáenz, which we've titled "Curers and Their Cures in Colonial New Spain and Guatemala: the Spanish Component." The paper deals with the training of physicians, surgeons, apothecaries and phlebotomists, and the regulations governing their practice. I was in Bancroft [Library] yesterday, getting Xeroxes of some pages. It's just a question of the last bit of finishing up. That's the most recent thing.

And I have been working almost a whole year on this damn Spanish translation of my first book on Tzintzuntzan, the Smithsonian monograph, *Empire's Children: The People of*

Tzintzuntzan⁶, being done by the Colegio de Michoacán in Zamora. They have the manuscript now and presumably are working on it. I'll check when I go to Tzintzuntzan in late May.

Riess: Did you find a new translator?

Foster: Well, they translated it so badly in Mexico that it would have been foolish to spend money printing it, so I was in despair until I found Roberto Gonzalez, a Mexican American, a very bright young man who was completing his Ph.D. here at Cal in anthropology, one of Laura Nader's students. He agreed to help me, so I hired him. He's been very good.

I don't know what I'm going to do next. I think I'll do a very brief autobiography, just for the National Academy, which wants something from every member. I've neglected that for years. And also for my kids.

[tape interruption]

And Faculty Lunching

Riess: Before we end, I want to review your lunch life. You know how dogged I am about all this! You had lunch with the emeritus anthropology group yesterday. Can you remember a couple of conversational themes?

Foster: Yes. The week before, Burton Benedict had mentioned an article in the *New Yorker* about the Trobriand Islands, about an Italian who had been on an island called Kitawa that none of them had heard about.⁷ Burton said he had sent a copy of the article to Raymond Firth, who knew [Bronislaw] Malinowski very well. The article in the *New Yorker* had said that Malinowski was a solid Catholic, and that just blew Raymond Firth apart. He said, "That's ridiculous." And it was.

And we talked about the problems of getting a--how sad it is that the department has to advertise for somebody to be chair, that we can't produce our own chairs. We [the anthropology department] are bringing a series of speakers who have applied for

⁶ Smithsonian Institution, Institute of Social Anthropology
Publication No. 6, 1948.

⁷See discussion p. 284.

the job, one of whom presumably will be selected. To my astonishment, some of the top contenders are from European countries, and Australia and India. I can't see how an anthropologist from a foreign country, where university administrations are very different from the United States, could possibly make a good chair. It would be hard enough for anyone from this country to step in cold, without being on the Berkeley faculty for a year or two before taking on the job.

Riess: Do you think that what they're looking for is someone to totally turn the department inside out and upside down and reshape it?

Foster: No, I don't believe there's any thought of that at all. It's just that nobody who has the ability is willing to serve as chair. Not everyone, clearly, has the administrative capacity to serve as chair. There are some faculty members who'd like to serve as chair, but who obviously aren't suited for the job. Stanley Brandes, our present chair, has done a marvelous job. If he would be a permanent chair, I think everyone would be delighted, but he says, "It's not fair for me to just have to go on." It's not good for the department, either.

Riess: Why is it so tough?

Foster: Your efforts are rarely appreciated until after you've been chair and you're being compared with the present incumbent, and then they think, well, so-and-so wasn't so bad after all, compared to this bastard or bitch we've got in now.

It's just a very hard job. You're not even first among equals, you're almost last among equals. So much of your effort goes to meeting competing offers and getting salary raises and the routine--it's just a very time-consuming job. It's the nature of the beast.

Riess: Are any of you emeritus people on the hiring committee?

Foster: No, no. I think none of the older--no, I think the committee is entirely composed of relatively young people in terms of years on the faculty. Alan Dundes and Laura Nader, for example, are not on the committee. They're the ones that have had the longest period of service.

Riess: Is it an issue that the university administration becomes involved with?

Foster: I think the university would prefer it if the department would come up with somebody. The university has never had to take over the administration of the anthropology department the way it has

sociology and some other departments, the art department, which had Stephen Pepper put in at one time to run it. We've always been able to settle our disputes so we never--I'm dumbfounded, really, that we're no longer able to run ourselves.

Riess: And your Friday group, the Little Thinkers, what did they discuss last week?

Foster: Well, Al Bowker's a great theater fan, and he goes to London once a year for the theater. Some of the other members are interested in theater. I'm not particularly. There was a lot of talk of a play called "Arcadia." I didn't really follow very well because I haven't seen the play, and I don't know the story, but it had to do with the question as to how much of the play was actually based on fact and how much was fiction. Bowker sent around an e-mail to the members, saying that he'd done some checking and found that it was authentic. [laughs] That's the first time that I can recall we've had e-mail communication on something we've talked about.

Riess: Was your father a club man?

Foster: No, he wasn't. He was a good Rotarian, but--well, in Ottumwa he belonged to the country club to play golf, but he was not a club person, a lodge type. Nor was Mickie's father. And none of the groups I belong to would be called clubs, really. They're just lunch groups.

The anthropologists have had various groups. Years ago, when we had a small department, we would usually eat at the Faculty Club. We had one table in front of the fireplace, and that was known as the Anthropology Table. If we had a guest we'd bring them. We had interesting people. And then we got too big, and something happened, we ceased meeting that way.

[tape interruption]

Rounding Out "This Whole Thing"

Foster: You asked for a rounding out of this whole thing. I suppose it would be best if I were rounding it out after the wedding of Christy, our oldest granddaughter, in Holland on the 16th of April, but that's beside the point.

The Ploughshares Fund is having its spring meeting in Washington, so Mickie is going. And Angela is also a member of the board. We're stopping there on our way to Holland. It's the

weekend before the wedding. The question is--Jeremy and I--are we going to stay, with Mickie and Angela at the same hotel, where they'll have double rooms? Or are we going to do something else? Mickie and I are staying with my sister the first night. We're going a day earlier. But she and her husband are going away, so I'll have to move someplace.

I thought of moving to the Cosmos Club, where I've been a member for more than fifty years. I used to stay there often when I went to Washington, but I've not been there to stay for a long time. I thought of staying there, but I wasn't sure that I wanted to, really. We were talking to Angela the other night, and she was speculating as to what Jeremy would do, and she said, "You know, I think he'd be interesting in seeing that--what was that club you took me to once?" I said, "The Cosmos Club." She said, "Yes, I think he'd like to see it."

I said, "Would he like to stay there if I stay there?" She said, "Well, I'll ask him." So he came on the phone, and I said, "Jer, would you like to stay at the Cosmos Club for a couple of nights before we leave for Europe?" He said, "I'd love it, Pop." That pleased me immensely. I think the love of children means more to you when you've known of times in the past when they were doubtful.

Riess: Thank you. We are finished for today, we've come to an end of the story. But after you return from the wedding, maybe you will add more to round things out.

[time passes]

Foster: And so here I am, six months later, "rounding out" what has gone before. It's been a busy half-year, both in professional and private lives. Christy's wedding was one for the books. I'll not say more than that she and her fiancé, Rob Burow, had rented Kasteel Wijenburg--really a very livable country home, in spite of being called a "castle"--near Nijmegen. They arrived in a coach-and-four, complete with liveried footman and driver, to the huzzahs of friends and family members.

Mickie and I were there from noon until 10 p.m., and the party was still going strong when we left. After being served a sandwich lunch we went upstairs to a large room for the civil ceremony, conducted by an attractive young woman, an official of the city whose jurisdiction included the castle. An hour later we followed the bride and groom a couple of hundred yards to the village church where the religious ceremony took place. Then back to the castle, and cocktails followed by an excellent dinner in the basement. Quite a bash.

The following morning the eighteen family members who had made the trip from the U.S. left with Wijbrandt and Melissa as guides for the island of Terschelling, one of several barrier islands in the North Sea, a two-hour ferry ride, where we spent three nights. Terschelling is a lovely, remote spot, a favorite vacation destination in summer, with wildlife preserves, walking trails, excellent birding, and good restaurants, one a one-star Michelin. Meanwhile, the newly-weds took a charter flight to Sri Lanka, a popular place for Dutch vacationists.

Riess: That was a grand wedding, and as you say, a half year has passed since April, and I know that a lot more has happened that you all are dealing with and trying to make sense of--if that is possible. Your own symptoms, which now have a name, and Mickie's illness and treatment. Is there anything you would say about this present difficult coda to your lives?

Foster:⁸ So here I am again, over a year since we began "rounding out" what's gone before. It's been a busy year, both in private and professional lives. Our "generic" letter that accompanied our Christmas card summarizes our non-professional experiences [see Appendix]. We had a wonderful week in Kauai with Jeremy and his family, where we received a telephone call from Holland informing us of the birth of Jasper Burow, our first great-grandchild. The thought has occurred to me that Jasper is the first person in the family with a good chance of living in three centuries. After all, he accomplished in the first nine days of his life what took me more than eighty-six years: to live in two centuries. If the science of medicine keeps on extending life in the 21st century as it did in the 20th, it is easy to expect young Jasper to live 100 years plus ten days!

Mickie is halfway through her chemotherapy, which she tolerates well and which appears to be doing what it is supposed to do: kill the cancer cells. My PSP progresses, as expected, and I had the interesting experience of being moved about airports in a wheelchair in Hawaii, and hoisted to the plane door in a sort of backhoe! I often think how fortunate it is that old age usually comes in small increments. By the time the next increment manifests itself, we have adjusted to its predecessor, and find that life is still worth living. I am sure that if old age appeared full blown in a single blow we would all commit suicide.

My professional life has been marked by both satisfying and disturbing experiences. On the satisfying side, Luz María

⁸Professor Foster added the following text during his review of the transcript.

Hernández and I completed our chapter, "Curses and Their Cures in Colonial New Spain and Guatemala: The Spanish Component," for a reader on colonial health practices being edited by our good friends Brad Huber and Alan Sandstrom. It is nice to feel that one's younger colleagues feel that a person in his mid-eighties still has something to offer, and equally nice to be pleased with the resulting product.

The Ishi Affair

Foster: On the disturbing side is the Ishi affair, which has brought me into conflict with some of my younger colleagues in the Anthropology Department. Ishi, as almost everyone knows, was the Yahi Native American who appeared near-naked and starving in an Oroville, California, slaughterhouse corral at dawn one day in 1911. Alfred Kroeber, who built the Berkeley department, sent his colleague, T. T. Waterman, to Oroville, where the sheriff had put Ishi in a jail cell to protect him from the curious crowds gathered to see the "wild Indian." Waterman returned to San Francisco with Ishi, who lived out the last five years of his life in the Anthropology Museum, at that time adjacent to the UC School of Medicine.

When Ishi died of tuberculosis in March 1916, his brain was removed during the autopsy performed on all patients dying in the university medical school hospital. The remains were cremated and interred in a pottery container in a cemetery in Colma, a suburb of San Francisco not far from the medical school. The whereabouts of the brain was forgotten, and it became a non-issue until 1997, when it was disclosed that Kroeber, who had opposed the autopsy, and was on sabbatical leave in New York City when Ishi died, had sent it to the Smithsonian Institution in Washington, D.C., eight months later, where it was added to the collection of primate brains of curator Ales Hrdlicka.

The fate of Ishi's brain immediately became headline news in numerous newspapers, and the source of editorials and cartoons, some using intemperate language, such as the *Contra Costa Times*, and including UCB Anthropology Department memoranda [see Appendix for copies of all memoranda]. On March 1 the question of a department statement on the Ishi affair was raised in faculty meeting, and a three-person committee was appointed to draft the statement. On March 3, 1999, the committee issued the first draft of its memorandum [#1 in Appendix]. I immediately called Nancy [Schepers-Hughes] and expressed my view that the memorandum was unfair both to Kroeber and his colleagues. We had an amicable



Dedication of the George and Mary Foster Anthropology Library, 1997.
From the left: Wijbrandt, Melissa, George, Mary, Jeremy, and Angela.

discussion, and Nancy told me that she thought I would feel better about a revision that had just been completed. She further invited me to offer suggestions for a third draft, an invitation I felt I could not accept. Instead, under date of March 12 I sent her a memorandum outlining my position [#2].

When the fourth and presumably final version of the statement was issued, I wrote a long commentary addressed to "Anthropology Faculty, UCB," dated March 24 in which I set forth my objections to the views expressed in the several versions of the department's position [#3]. The final version appeared in the *Anthropology Newsletter* for May, over the signature of Stanley Brandes, Chair [#4]. Brandes did not agree with the statement but felt that as department chair it was his duty to submit it for publication.

Meanwhile, an editorial in the *Contra Costa Times* on March 26 reported that the Smithsonian Institution refused to return Ishi's brain to a group of California Native Americans that wanted to bury it along with his ashes which still remained in the Colma cemetery. The editorial continued, "This is the final indignity heaped upon Ishi by anthropologists. When he came out of the hills into Oroville in 1911, they made him a curiosity. They studied him like a rat in a cage. Then, when he died of tuberculosis, they carved him up and sent his brain to the Smithsonian."

I felt that both the department's statement and the *Times* editorial were grossly unfair to Kroeber and his associates, that the anthropologists' behavior was entirely ethical, judged by standards of the time, and that apologies were unnecessary. Many of my colleagues shared this view, including all who knew Kroeber and Edward Gifford and his wife, Delila, the only people still living in the 1950s who knew Ishi well. I felt compelled to reply to the charges of my colleagues against a man no longer able to defend himself. Consequently I spent much of the summer of 1999 preparing a reply to the published version. My original draft of 4,600 words was drastically cut to 1,800, appearing as "Responsibility for Ishi" in the October 1999 *Anthropology News* [#5]. Karl Kroeber and Ursula Kroeber Le Guin (Alfred's children) also both weighed in with letters in the same issue defending their father.

The editor of the *News* advised me she had received a veritable "tsunami" of complaints from Berkeley. "I have received several rather agitated complaints against our decision to publish your piece. I've responded (as I do to any indignant reader) that the AN doesn't practice politics, and that we make an effort to air as many sides to an issue as possible... I imagine that there

will be a letter to the editor for the December edition." However, the only thing that appeared in that edition is a "Correction" (p. 4) pointing out that editorial cutting in the East had omitted the word *Times* before the date "March 26, 1999," thereby giving the erroneous impression that a quote from the *Contra Costa Times* was from a UCB memorandum.

I have received letters from colleagues who share my views on the question of the ethics involved in the treatment of Ishi. These include Evon Vogt, Gordon Willey, and James Watson at Harvard, Harold Conklin at Yale, Dell Hymes at Virginia, Herbert Lewis at Wisconsin, and John Ingham at Minnesota. Comments such as these appear in their communications: "It was a clear and forceful statement... There is so much soft-headed thinking these days about what earlier anthropologists did or did not do to or with informants, and it was high time it was answered," "It continues to amaze me that our field gives every group in the world the benefit of the doubt...except our own predecessors. As [if] three generations ago in the United States was not a different culture," "It does seem to me that 'presentism' has become entirely too fashionable in contemporary anthropology," "Bravo!... There's too much so-called 'ethnic correctness' these days in our sprawling profession," "It was good to read your compassionate and sensible response to the Ishi controversy," "'Responsibility for Ishi'...is important both for the honor and understanding of...Kroeber and Ishi, and for American anthropology as a whole. The Berkeley statement is symptomatic of so much that is wrong with scholarship today," et cetera.

My colleague, Professor Emeritus Gene Hammel, beautifully sums up the argument in powerful defense of Kroeber and Gifford in a letter entitled *Eating the Dead*. He points out that compassion for those it studies has been a dominant theme in anthropology's history, and that on the whole anthropologists have defended and protected their hosts. "I think of Kroeber and Gifford as Ishi's protectors." Hammel continues, "What is most distressing in the controversy about Ishi...is the fury of the attack on anthropologists who, as best they could, and as fallible as they might have been, extended this compassion. What is distressing is the intolerance of failure to exhibit political perfection in the past by current definition, the disregard of historical context." Hammel, who worked for Gifford as a preparator in the UC museum, and who knew Kroeber, praises both men for their treatment of others. He concludes with, "As I think about those people...I cannot fathom the ignorance, intolerance and ahistoricity of those who attack Kroeber and Gifford, nor can I fail to remark on their classic attempts to gain status by eating the dead." (*Anthropology News*, December 1999, p. 4) [#6].

So ends the year 1999, the century, and the second millennium, all three of which have been very kind to me and those I love so dearly.



A REALIZATION



Offered without comment by George Foster

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Born: October 9, 1913, Sioux Falls, SD. Wife: Mary LeCron Foster. Two children.

Education:

1935 B.S. Degree, Northwestern University (Anthropology).

1941 Ph.D. Degree, University of California, Berkeley (Anthropology).

Professional Employment:

1941-1942. Syracuse University, Syracuse, NY. Instructor in Sociology.

1942-1943. University of California, Los Angeles. Lecturer in Anthropology.

1943-1952. Ethnologist, Smithsonian Institution, Institute of Social Anthropology.

In charge of Mexican branch and Professor, National School of Anthropology, Mexico City, 1944-1946; Institute Director in Washington, D.C., 1946-1952.

1953-1979. University of California, Berkeley. Professor of Anthropology.

Department Chairman, 1958-1961; 1972-1973. Lecturer in Public Health,

1954-1965. Director, Ph.D. Program in Medical Anthropology (jointly offered with Univ. of Calif. Med. School in San Francisco), 1972-1979.

1979-Present. Professor Emeritus.

Anthropological Field Research:

1937 (Summer). Yuki Indians, California.

1940, 1941 (Spring). Popoloca Indians, Veracruz, Mexico.

1944-1946. Peasant Research in Tzintzuntzan, Michoacán, Mexico.

1949-1950. Field research in Spain on Spanish background of contemporary Spanish-American culture.

1958 - Present. Continuing longitudinal study of socio-economic change in Tzintzuntzan, Michoacán, Mexico.

Professional Societies:

American Anthropological Assn. (Executive Board, 1957-1960; President, 1970);

Society for Medical Anthropology (Executive Board, 1970-1973); Society for

Applied Anthropology; American Ethnological Society; Southwestern

Anthropological Assn.; Sociedad Mexicana de Antropología.

Principal Professional Interests:

Peasant and Rural Societies; Social and Economic Change and Development; Medical Anthropology; International Health.

Honors:

Member, U.S. National Academy of Sciences; Member, American Academy of Arts and Sciences; Guggenheim Fellow (1949-1950); Fellow, Center for Advanced Study in the Behavioral Sciences (1969-1970); Recipient, Berkeley Citation (1979); Doctorate of Humane Letters, Southern Methodist Univ. (1990); Distinguished Service Award, American Anthropological Assn. (1980); Distinguished Research Award, Southwestern Anthropological Assn. (1981); Malinowski Award, Society for Applied Anthropology (1982).

Major Consultancies, Seminars, Workshops, etc.:

- 1951-1952 (Oct.- May). Member, USPHS -Institute of Inter-American Affairs Evaluation Team,
"The First Ten Years of Bi-lateral Health Programs in Latin America."
- 1952 (May). Geneva, Switzerland. Technical Adviser to the American Delegation to the Fifth
World Health Assembly.
- 1954-1956. Washington, D. C. Member, Health Committee, U. S. Foreign Operations
Administration.
- 1955 (June-Sept.). India, Pakistan, Philippines. Community Development Evaluation, U. S.
Foreign Operations Administration.
- 1956 (Summer). Guatemala City. Participant, "Seminar on Social Integration."
- 1957 (Mar.-June). Afghanistan. Consultant on Community Development, U. S. International
Cooperation Administration.
- 1957 (December). Endicott House, Dedham, MA. Participant, MIT - International Cooperation
Administration Community Development Seminar.
- 1961 (August). Burg Wartenstein, Austria. Wenner-Gren Symposium "Ceramics and Man."
- 1961 (Sept-Oct.). Lusaka, Northern Rhodesia. USAID Consultant on Community Development .
- 1962 (Jan-June). " " " " " " " " " " " " " " " "
- 1963 (August). Mexico, Bolivia. OAS Consultant on Community Development.
- 1964 (October). Mexico. OAS Consultant on Community Development.
- 1964 (July-Aug.). Endicott House, Dedham, MA. MIT-USAID seminar "Problems of Agricultural
Production in Developing Countries."
- 1964 (December). Washington, D.C. NRC-NAC International Seminar, "Nutrition and the
Pre-School Child."
- 1964-1966 Assn. of American Medical Colleges. Member, Advisory Committee , Study of Medical
Education in the Developing Countries."
- 1965 (July-Aug.). Kathmandu, Nepal. USAID Consultant on Community Development.
- 1966 (November). New Delhi, India. Third World Conference on Medical Education.
- 1967 (September). Burg Wartenstein, Austria. Wenner-Gren Symposium "The Teaching of
Anthropology in Latin America."
- 1967-1968 Washington, D.C. Member, NRC-NAS Advisory Committee on Government Programs
in the Behavioral Sciences.
- 1970 (Feb.) Washington, D. C. Pan American Health Organization, Member, Advisory Panel on
Eradication of Aegis aegypti in America.
- 1970-1973 Washington, D.C. Member, Behavioral Sciences Training Committee, National
Institute of General Medical Sciences.
- 1971 (Sept.) Cairo, Egypt. Participant, "Workshop on Human Settlements in New Lands: Their
Design and Development."
- 1971-1973 Philadelphia. Commissioner-at-large, Commission on Foreign Medical Graduates.
- 1973 (Jan.-Feb.; July-Aug.) Indonesia. Consultant on Health Education and Family Planning,
APHA-USAID
- 1974 (Jan-Feb.). " " " " " " " " " " " "
- 1975 Jan. - Feb.) Sri Lanka. Consultant on Health Education, WHO-SEARO.
- 1975 (Aug.) Burg Wartenstein, Austria. Co-organizer, Wenner-Gren Symposium "Theoretical
and Methodological Implications of Long Term Field Research in Social Anthropology."
- 1975 (November) Washington, D.C. Participant, USAID Workshop on "Tradition, Behavior and

Health."

- 1976 (March; Aug. - Sept.; Oct.) Geneva. UNICEF/WHO Consultant, JCHP Project "Local Participation in the Delivery of Primary Health Care."
- 1978 (Oct. - Dec.) Kuala Lumpur, Malaysia. WHO-WPRO Consultant on Health Education.
- 1979 (Oct. - Dec.) New Delhi, India. WHO-SEARO Consultant on Health Education.
- 1979 (November) Kathmandu, Nepal. Participant, Second International Workshop on Leprosy Control in Asia.
- 1980 (September). Washington, D.C. Plenary Paper "Community Participation in Health Education." Technical Discussions of Directing Council of Pan American Health Organization.
- 1980 (Oct. -Nov.). New Delhi. WHO Consultant in Health Education. Organizer, Consultative Meeting, "Social Science Research in Health Education and Community Participation."
- 1981 (March). New Delhi. Participant, "Rheumatic Fever in the 1980s - An Indo-U.S. Conference Workshop."
- 1981 (Aug. - Oct.). New Delhi, Bangkok, Jakarta, Colombo. WHO Consultant on Health Education. Organizer of the following meeting:
- 1981 (December). New Delhi. WHO Consultant. Conducted Consultative Meeting "Appropriate Technologies for Behavioural Science Research on Health Problems."
- 1982 (June). Bangkok. Participant, WHO-Mahidol University meeting on "Sociocultural Aspects of Food Safety."
- 1982 (Sept. - Oct.). Nigeria, Cameroon, Kenya, Geneva. WHO consultant on Socioeconomic Research on Tropical Diseases, and meeting of Expert Committee on "New Approaches to Health Education in Primary Health Care."
- 1983 (March). Manila. Meeting of the WHO-WPRO Sub-Committee on Behavioral Science and Mental Health.
- 1983 (June). Geneva. Participant, Joint FAO/WHO Expert Committee meeting on Food Safety.
- 1983 (November). Ulm, West Germany. WHO-EURO Workshop on "Scientific Analysis of Health and Health Care Paradigms, Methodologies and Organization."

George M. Foster
Publications
June 1, 2000

- 1939 (With Walter Goldschmidt and Frank Essene), War Stories from Two Enemy Tribes. *Journal of American Folklore* 52:141-154
- 1940 Notes on the Popoluca of Veracruz. Mexico, D.F.: Instituto Panamericano de Geografía e Historia, Publ. No. 51.
- 1941a From a Mexican Diary. *International Quarterly*, Winter, pp. 33-36.
- 1941b String Figure Divination. *American Anthropologist* 43:126-127.
- 1942a A Primitive Mexican Economy. New York: J. J. Agustin. American Ethnological Society, Monograph V.
- 1942b Indigenous Apiculture Among the Popoluca of Veracruz. *American Anthropologist* 44:538-542.
- 1942c Ordeal by Vapor. *Bulletin of the Pan American Union* 76:443-446.
- 1942d Review of Stig Ryden, A Study of the Sirionó Indians. *Boletín Bibliográfico de Antropología Americana* 6:137-138.
- 1942e Review of Alfred Metraux, The Native Tribes of Eastern Bolivia and Western Matto Grosso. *Boletín Bibliográfico de Antropología Americana* 6:138-139.
- 1943a The Geographical, Linguistic, and Cultural Position of the Popoluca of Veracruz. *American Anthropologist* 45:531-546.
- 1943b Review of E. F. Castetter and W. H. Bell, Pima and Papago Indian Agriculture. *Pacific Historical Review* 11:85-86.
- 1944a A Summary of Yuki Culture. Berkeley: University of California Anthropological Records 5(3):155-244.
- 1944b Primitive and Machine-Age Productivity. *The American Journal of Economics and Sociology* 3:583-598.
- 1944c Nagualism in Mexico and Guatemala. *Acta Americana* 2:85-103.
- 1945a Sierra Popoluca Folklore and Beliefs. Berkeley: University of California Publications in American Archaeology and Ethnology 42(2):177-250.
- 1945b Some Characteristics of Mexican Indian Folklore. *Journal of American Folklore* 58:225-235.

- 1945c Review of Alexander and Dorothea Leighton, *The Navaho Door*. *América Indígena* 5:90-91.
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GRADUATE STUDY AT BERKELEY 1935-1941

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Walter Goldschmidt and I arrived in Berkeley in mid-August 1935 to begin our doctoral studies, he from the University of Texas, and I from Northwestern. For the next six years we were colleagues and close friends, members of a small group of anthropology graduate students all of whom were molded by the same formal and informal educational forces, and subject to the same social and cultural pressures. It is hard to realize what a world apart Berkeley was at that time. Although the DC-3 airplane began transcontinental service while we were students—New York then became only 18 hours away—air travel was expensive, and rail and highway were the normal modes of travel. Chicago was a 60 hour trip, and the east coast 24 hours more. West of Omaha there were few paved roads and the drive, although fairly routine, was subject to delays because of rain, and tire and motor troubles rarely encountered today. Anthropology was unknown at Stanford and UCLA, and the Department at Washington was 36 hours away by rail, and longer over unpaved roads. The Annual Meetings of the American Anthropological Association always were held in the east or midwest, just after Christmas, and none of us, to my knowledge, ever attended. We lived, in a very real sense, in an isolated world, members of a culture remote from other anthropological cultures known to lie to the east and north. Consequently, we learned anthropology from publications, our teachers, and each other, largely unaffected by first hand contact with colleagues in other parts of the country.

In 1935 the Anthropology staff consisted of Alfred L. Kroeber, Robert H. Lowie, Ronald Olson, and Edward Gifford. The Department was housed in "The Old Tin Barn," a corrugated steel two-story building located about 100 yards north of Kroeber Hall. This structure had been built shortly after the turn of the century by Mrs. Phoebe H. Hearst—

anthropology's early patron—to house California Indian specimens, and the rich collections being sent to Berkeley by Max Uhle and G. A. Reisner, whose research was also supported by Mrs. Hearst. It survived until 1953, when it was razed to permit construction of the School of Music's Hertz Hall. Most of the ground floor was open to the roof, piled high with sarcophagi, canoes, a totem pole and other bulky artifacts. Several offices, including the "bullpen" for teaching assistants, lay adjacent to this central space. The offices on the second floor opened onto an interior balcony overlooking the central well, and included the Departmental Office (a 10 by 15 foot cubbyhole), and rooms for Kroeber, Lowie and Olson. There were also two small seminar rooms and, reached by an outside staircase, a larger room with a capacity of about 100 students, used for most Upper Division courses.

A good deal of graduate study took place in a seminar room on the third floor of the main library, where Bureau of American Ethnology Annual Reports, the University of California Publications in American Archaeology and Ethnology, and other standard works were shelved. Graduate students had keys to this room, which was available to them except when Kroeber and Lowie held seminars there.

The Museum of Anthropology, cared for (and later directed by) Edward Gifford, was the third center of anthropological activities on the campus. It was housed in a fine Victorian three-story structure built during the 1880s, which had until shortly before our arrival housed the College of Engineering. In an age of few fellowships the several preparatorships available to graduate students were much coveted sources of support. The two small exhibition rooms of the Museum were open only twice a year, for two weeks in the middle of the fall and spring semesters, when artifacts appropriate to lecture courses were placed on exhibit.

In 1935, and for several years thereafter, graduate seminars were given only by Kroeber and Lowie. Although Olson had completed his Ph.D. degree in 1929 and had been on the staff since that time, he was not deemed sufficiently mature to be trusted with a graduate seminar, until 1937! Gifford, a remarkable, self-trained anthropologist, whose most advanced degree was a high school diploma but who ultimately rose to full professorship, did not give graduate seminars until even later. We had come to Berkeley, all of us, to study with Kroeber and Lowie.

Anthropology was a depressed field in 1935, with very few jobs, and departments, even as today, were discouraging applicants. When I decided in the spring of that year to attempt a graduate career I wrote Kroeber asking about admission. Lowie replied, explaining that he was Departmental Chairman for that year, and telling me how unwise it was to consider graduate work in anthropology. He also called to my

attention certain hurdles: before being permitted to take graduate courses students had to pass a reading examination in French and German (without dictionary) and a three-hour general examination in history, biology, zoology, and the like. My persistence, rather more than academic promise, probably accounts for my acceptance.

When Wally and I arrived in Berkeley we were given a seven-page mimeographed statement that described graduate work at the University, and indicated the principal fields of anthropology, and the requirements in each. A long list—an "absolutely irreducible minimum of reading"—was liberally sprinkled with such names as Von den Steinen, Norden-skiöld, Laufer, Bogoras, Thurnwald, Kirchoff, Hahn, Cooper, Matthiasen, as well as others better known by today's students. French and German titles appeared throughout the list. I had had the good fortune to pick up enough French in high school and college to pass that examination, but German was another matter. I found a tutor, Herr Müller, a native German and graduate student in History, and settled down to a routine of three hours of German study five days a week. Twice during the winter I took the examination, failing it both times. I was encouraged only by the fact that my fellow students were doing little, if any, better. Finally, just before the end of the spring term Lowie approved my third attempt, several paragraphs from Merker's *Die Masai*.

Passage of all required examinations did not, however, mean that students were now eligible for *any* graduate seminar. Prior to that, we were required to take Anthropology 206, the "Pro-Seminar." Kroeber met a half-dozen of us on the first scheduled day of the class, and set the semester's assignment: Take B.A.E. Annual Report No. 24, Stewart Culin's *Games of the North American Indians*, and classify them. He would meet us on the final day of the semester, he said, to collect our reports. This seminar was highly useful. *Games* was a monumental, 800 page volume describing particularly dice and hand games, with no order, theory, or interpretation whatsoever. By wading through Culin we learned one of our first major lessons, the importance of classification.

Graduate training at Berkeley was fairly "standardized," since we took almost the same courses. Kroeber and Lowie each gave two three-hour weekly upper division lecture courses, plus a graduate seminar. Kroeber's usual courses included Anthropology 137, "The Indians of California," and Anthropology 103A-B, a monumental two-semester course entitled "The Growth of Culture" which ranged the world from ancient Peru to Mesopotamia to China. Lowie gave Anthropology 102, "Chapters in Culture History," dealing with the origin and development of agriculture, domestic animals, social forms, and primitive technology. His chef-d'oeuvre, however, was Anthropology 101A-B, in which he

covered the main ethnographic facts of six continents plus the islands of the Pacific. All graduate students took these courses, and by the time we had completed them, we knew at least something about most of the world's peoples, past and present, known to anthropologists at that time. After these courses, plus graduate seminars, we needed relatively little additional work for the two "fields" which we all offered for the Ph.D. qualifying examination, "World Ethnography," and "History of Anthropological Theory," in addition to a chosen third field.

Since only two graduate seminars were given at any time, we usually found the same group of students together. Lowie's "History of Ethnological Theory" continued, year after year, with each semester devoted to a different anthropologist. I remember particularly the semester when we studied Malinowski who, in spite of great differences in views of anthropology, was a close friend of Lowie. Over the years Lowie methodically assembled in this seminar the data and ideas that appear in *The History of Ethnological Theory*. Kroeber's seminars ranged more widely. In addition to the Pro-Seminar I remember one on linguistics, in which we studied Japanese, and in which I learned the little I have ever known about formal linguistics. I don't recall that the phoneme was ever mentioned, although Bloomfield had been in print for several years.

During the pre-war years the Berkeley Ph.D. program would have to be described as "old-fashioned," even for that time. It was rigorous—perhaps to a fault—and provided its survivors with a magnificent background of anthropological information, but it largely ignored new developments taking place in other leading departments, such as British social anthropology at Chicago, culture and personality explorations at Yale and Columbia, and the American social approach set forth by Linton in *The Study of Man*. At Berkeley anthropology was very much an historical discipline, in spite of the fact that the mimeographed outline previously mentioned says "cultural anthropology is a social science." It was the only context, I believe, in which we were exposed to this novel idea; certainly it never intruded in course or seminar work. The cultural geographer Carl Sauer was the principal outside source of stimulation for most anthropology graduate students, and rare was the one who did not take at least one of his seminars. I recall with particular satisfaction the things he taught us about the history of domesticated plants and animals. In travelling about the world in later years, I have derived great pleasure from knowing where the foods I have eaten and the animals I have observed originated, a pleasure many of today's young anthropologists miss because of ignorance of this kind of history.

Historical reconstruction was viewed as the basic task of anthropology. Consequently, we studied culture areas intensively, and noted the

strengths and defects of the age-area hypothesis as a research tool. The question of independent invention vs. diffusion was very much alive, as was the origin of totemism. To speak authoritatively on these topics we assembled long lists of "traits" and "complexes" such as the distribution of matrilineal and patrilineal clans, moieties, sinew-backed bows, other items of material culture, and practically everything else one could think of. I recall that theory and problem-orientation played very little role in our training, at least overtly, which seems strange in view of the major theoretical contributions of Kroeber and Lowie, and of some of their students. Wondering if I remembered poorly, I returned to my course notes and found that my memory was indeed accurate.

Lowie's lecture on April 29, 1936, in "Chapters in Culture History," is illustrative. Discussing the Ghost Dance of 1890-91, Lowie pointed out that this type of phenomenon had existed since the 17th century in North and South America, that it started with the Paviotso and spread to the Sioux, arriving among the latter at just the right psychological time to be taken up, since they were in desperate circumstances. They re-interpreted the doctrine, originally peaceful, and gave it a war-like cast. Different tribes reacted differently to the Ghost Dance, but within each there were three main types of reaction: some people were fervent believers who had additional visions, others were simply fervent believers, and still others were skeptics. "The diffusion of this dance gives excellent examples of how automatically a tribe adopts a new idea to fit its own culture pattern, rejecting some features, adding others, and reinterpreting still others." Lowie went on to tell us that messianic cults are good examples of how cultural features can be invented more than once. Thus, the same type of reaction occurred among the Zulu about 1840, and for the same reason as in North America: the pressure of Europeans. There, as in North America, the idea of elimination of the whites was prominent. There were similar cases in South America as early as the 17th century—true parallelism, said Lowie. "This also is an example of how economic reasons are not the only determinants of human action. Thus, among the Zulus, a prophet ordered his followers to destroy all of their cattle—an unthinkable thing. And yet these people destroyed thousands of cattle under strong religious excitement."

Lowie clearly was still thinking largely in terms of the earlier problems of diffusion, independent invention, parallelism, and giving the lie to economic determinism. He recognized the importance of reinterpretation of traits when adopted by groups that previously had not had them, and he noted personality differences and differential acceptance of the new. Yet in spite of Thurnwald's influence on Lowie, and his colleague Gifford's use of the term as far back as 1927, the word "acculturation"

appears no place in my notes for this lecture. Clearly there was plenty for us to think about, but we had to pull the lessons together for ourselves.

The factual nature of learning is further illustrated by upper division course examinations. In reviewing those I took I am struck with the preponderance of questions dealing with the presence or absence of traits, their distributions, and the time sequence of their appearance. Examinations always were "objective," with words or phrases to be underlined or inserted in blank spaces in sentences. In one question on the Indians of California, Kroeber asked:

State the occurrence of the following 38 traits in terms of these five California areas: Colorado River, Southern, Central, Northeast, Northwest.

Among the 38 traits were basketry cap, hair net, sandal, sweat-house lacking, plank-built canoe, gourd or turtle-shell rattle, iris fibre string, women's clan names, no marriage of widow by husband's brother, no scalps taken and "pit roasting of adolescent girl."

Two questions from Lowie's Chapters in Culture History follow, with the correct answers underlined or filled in.

A hunting method illustrating social cooperation is that of (Group Drives). It was found a few centuries ago among the (Lapps) of Europe and among the North American Indians of the (Plains) and (McKenzie) areas when hunting such game as (buffalo) and (caribou).

The spread of the cultivated banana (implies, does not imply) human agency because (side shoots must be cut and planted). The same (holds, does not hold) for the coconut because (it germinates naturally, even after long periods of sea drifting).

Reflecting the character of its two leaders, the Department of Anthropology during the pre-war years was very formal. We did not stick our heads into Kroeber's and Lowie's offices and shout, "Are you busy?" as today's students do with their professors. Kroeber was protected by the formidable Mrs. Chilcote, who was the Departmental secretary and entire "non-academic staff," and no student passed her without good reason. At the beginning of each semester we were permitted a few minutes with Kroeber and Lowie to discuss our programs, but I do not recall ever sitting down in either of their offices until after I had passed the qualifying examination. It is not that they were unfriendly. Rather, with heavy teaching loads and prodigious research and writing programs, they simply did not have time to banter with students. So it was to Edward Gifford and his wife Delila, and Ronald Olson and his wife Marie, that graduate students turned for informal faculty contact. They

were the ones that invited us for Thanksgiving dinner, or for spur-of-the-moment parties in their homes. They were the ones we addressed by their first names, and to whose offices we could go for counsel and advice. During much of this period Paul and Doris Radin lived in Berkeley. Paul enjoyed having graduate students drop in, and we profited greatly from informal discussions with him. Intellectually unassuming and unpretentious, he had the knack of treating us as equals, as full professionals and not as students. In addition to these people, Wally early made friends with two outstanding economists, Paul Taylor and the late Lloyd Fisher, both of whom played major roles in directing his interests to the problems of contemporary society. It was not until many years later when on a community development assignment I traveled around the world with Paul Taylor that I realized what I had missed as a student.

The graduate student body during the late 1930's was small and fairly cohesive, and included such people as Homer Barnett, Philip Drucker, Margaret Lantis, Katherine Luomala, Harold Driver, Omer Stewart and Frank Essene. Bob Heizer and the late Harry Tschopik, senior undergraduates, for all intents and purposes were treated as graduates. Theodore McCown, still a graduate student, was in England completing the manuscript of his Mt. Carmel materials; he returned to Berkeley in 1938, joining the permanent faculty after completing his degree the following year. Other well-known anthropologists had completed doctoral studies shortly before we arrived, and their names were still very much a part of Department conversations: Ralph Beals, Julian Steward, Dorothy Lee, Isabel Kelly, and Cora DuBois, to name a few. The stories we heard led us to believe we had arrived in Berkeley just at the end of the Golden Age of graduate study; our lives seemed humdrum in comparison with the Bohemian life that had gone on before.

During this period Ronald Olson was drawing huge introductory anthropology classes of 500 or more students for the entire year. This was fortunate for the graduate students, for it meant at least six teaching assistantships every semester, highly valued because fellowship support was rare. The going rate of \$600 a year for the privilege of meeting six sections a week for thirty weeks was considered very generous. Most of the Berkeley anthropologists of the 1930's proved to be good teachers; I think the confidence and experience they acquired as teaching assistants was largely responsible.

The T.A.s' bullpen was a meeting place not only for the T.A.s themselves, but for other students, and here we had many good sessions. As a group we were tremendously supportive of each other. It occurred to no one to conceal ideas or data, and it astonished me when I returned to Berkeley many years later to find that anthropology was regarded by

many graduate students as a limited good, so that one had to be cautious in discussing data and ideas with fellow students and faculty lest they be "stolen." We read and criticized each other's papers, and learned much in the process. My first published paper, "War Stories from Two Enemy Tribes," was coauthored with Wally Goldschmidt and Frank Essene. None of our professors helped us with this endeavor. Rather, it was Margaret Lantis, a couple of years farther along in her studies, who helped us put the manuscript into a form acceptable to *The Journal of American Folklore*. This act was, I think, typical of the relationship between students at that time.

Since graduate student records were almost non-existent, planning for the qualifying examination was somewhat arbitrary. After we had been familiar faces around the Department for three or four years, Lowie—but more likely Kroeber—would stop us in the hall and say, "I think you'd better take your 'writtens' next month." The "writtens" were, to put it mildly, an ordeal. We wrote for six hours a day for five days, on our three fields, and on anthropology in general. Not infrequently students were failed on one or two days' work, and required to retake those parts after more preparation. Questions were handed out to us at the beginning of each three-hour session. When I appeared on the morning of September 14, 1939, I was given a slip of paper with the following:

- I. Describe or comment on the significance of the following:
 - a. Jesup Expedition
 - b. Torres Straits Expedition
 - c. Tylor "On a Method etc."
 - d. Kirchoff "Die Verwandtschaft der Urwaldstämme Sudamerikas"
 - e. Rivers "Kinship and Social Organization"
 - f. Von Den Steinen's Xingu River Expeditions
 - g. Radin "Primitive Man as Philosopher"
 - h. Laufer "Sino-Iranica"
 - i. Morgan "Systems of Consanguinity etc."
 - j. Lila O'Neal "Yurok-Karok Basket Weavers"
- II. Comment on the significance and distribution of the following:

<ol style="list-style-type: none"> a. Bronze b. Twin infanticide c. Balsa d. Bull roarer e. Bark cloth 	<ol style="list-style-type: none"> f. Matrilineal descent g. Matrilocal residence h. Possession i. Buffalo (carabao) j. Acorn eating
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After nine more sessions like this, the three-hour oral examination held few terrors.

While it was assumed that an anthropologist would experience field work as a part of doctoral training, there was no necessary correspondence between that experience and the doctoral dissertation. Heizer, for

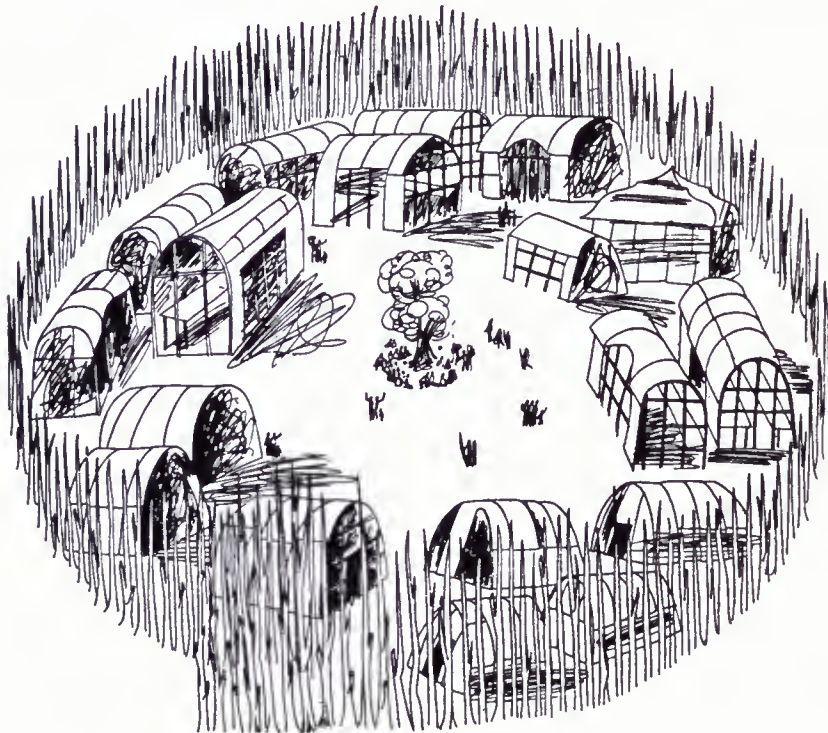
example, did sufficient archaeological research in California, Nevada, and Alaska for at least three dissertations, yet he did a library thesis on aboriginal whaling in the Pacific. Almost all cultural anthropologists did field research among the California Indians, for Kroeber was still anxious to fill in the gaps in the aboriginal record. During the summer of 1937 I was given \$200 and sent to Round Valley to study the Yuki. The same summer Wally and Gale Goldschmidt were sent to the Hupa Reservation. One of my pleasant recollections of that summer was spending a couple of days with them in Hupa, and witnessing the White Deerskin dance.

We received absolutely no preparation for field research. There were no courses on research design or field methods, and no instructions about how to live in the field. It was assumed that by reading ethnographies we would learn what we had to do. When Kroeber told me I was to go to the Yuki, I had more than a few doubts as to how to go about it. "Professor Kroeber," I asked, "can't you give me some advice about field work?" His eyes twinkled, he paused a moment, and then said, "I suggest you get a stenographer's notebook and a pencil." Then he marched on down the hall. I have often told this episode, and have been amused to note that increasingly it appears in accounts of early field work. Yet somehow most of us made it. While at the time we took great pride in the accomplishment, we later began to realize there are more efficient and more humane ways to prepare students for the field.

For those of us who worked among California Indians, our assignment was traditional: learn everything possible about aboriginal life. When I was among the Yuki, a graduate student from Columbia University arrived to study the Indians as they lived in 1937. I was faintly amused, and not a little disdainful, at the thought of such a non-anthropological topic. In looking back I am surprised that it was never suggested to us that we go to Africa, India, or Oceania for a year or two, to do an intensive tribal or community study. This had become standard practice for students at Yale, Columbia, and Chicago, but field research at Berkeley was still viewed as consisting of one or a series of fairly short trips. My own doctoral research among the Popoluca Indians in Mexico, for example, took only about three months, although I spent another five or six months in the country learning Spanish and studying Mexican ethnography, for which I had no training in Berkeley. Wally Goldschmidt must have been the first one of us to do a true community study, his work in Wasco in the San Joaquin Valley, but it was not a project for which he received much encouragement from his professors.

The pre-war period of graduate study, and its distinctive style, were brought to a close by Pearl Harbor. During the war there were few

students, and with peace both staff and the forms of graduate study began to change significantly. Kroeber retired in 1946, and David Mandelbaum was brought in to replace him, with John Rowe following in 1948. With the G.I. bill of rights and the surge of interest in anthropology that followed the war, the Department expanded rapidly in faculty and numbers of graduate students. Those of us who were there in the thirties look back with nostalgia at a simple, sylvan time. It was a good world, and a happy life, and we dreaded the thought of having to cut the umbilical cord to take a job. I was spared the usual agony. In early September 1941 I had just finished my dissertation, and was wondering what the future held. Kroeber called me to his office and asked if I would like a job. I said indeed I would. "I have just learned by telephone of a one-year job at an Eastern university," he said. "It's not on the seaboard, and it's not one of the Ivy League schools. The opening is in sociology. Will you take the job?" "Yes," I said, "where is it?" "Syracuse University," he replied. Two days later I was flying east, with little opportunity to feel sad about the end of six of the happiest years of my life.



NOTES ON UCB ANTHROPOLOGY DEPT. HISTORY
Oct. 28, 1995

The following items come from my line-o-day books.

1955

Sat., May 21, Edward Gifford Retirement dinner at Spengers. I Toastmaster.

Sunday, Oct. 23. 5.5 Earthquake at 8:00 P.M.

Tues., Dec. 13. Meeting of Anthropology-Art Building Committee (After my appointment as prof. effective 7/1/55 I became Departmental representative on this committee.)

Fri., Dec. 16. All day at office working on new Museum plans.

Sat., Dec. 17. A.M ditto.

Sun., Dec. 18. From 11:00 A.M to 3:00 P.M., ditto.

Mon., Dec. 19. Staff meeting on new museum plans.

Tues., Dec. 20. Anthropology-Art Building Committee meeting.

1956

Fri., Jan. 6. Robert Lowie talks on "Early Field Work" at Faculty Club, Stanford-Calif faculty evening.

Mon., Feb. 6. Meeting with University architects on new museum.

Wed., Feb. 15. Meeting with architects.

Wed., Feb. 22 to Wed., Feb. 29. Eric Douglas and wife in Berkeley advising on museum design.

Thurs., Mar. 1. As graduate adviser I invited the graduate students to 463 Kentucky Ave. to discuss curriculum. First time this was ever done. They enormously appreciative. Ditto Fri., Mar. 16.

Sat., April 4 - May 1. Mickie and I make museum study trip: Santa Fe (Folk Art Museum); Denver (Denver Public Museum); Milwaukee (Milwaukee Public Museum); Cambridge (Peabody Museum); New York (AMNH); Philadelphia (University Museum); Washington (Smithsonian); Tucson (Arizona-Sonora Museum).

Sun., April 29 - Tues., May 1. Tucson, AAA Executive Board Meeting: Pres. Haury; pres-elect Hoebel; Cole, Spoehr; Hallowell; Jennings; deLaguna; Secretary Bill Godfrey and editor Wally Goldschmidt.

Thurs., May 3. Work over new museum plans.

Fri., May 11, Meet with Art-Anthropology architects.

Fri., May 18. With UC architect Takiuchi to Gardner Dailey's San Francisco Office to see Art-Anthropology building plans.

Fri., May 25 Art-Anthropology Building Committee meeting.

Thurr., June 7. Mickie finishes Northwest Coast Art Exhibit in Telegraph Ave. windows.

Wed., June 13. Birthday party at McCowns' for Robert Lowie.

Sat., July 7. I start work on my "Spanish ms." Until Sept. 9 most days grinding on this book.

Thurs., July 12. Talk to Bob Ratcliff about house problems. Build or remodel?

Tues., July 17. Check new museum plans.

Thurs., Aug. 2. P.M. at the museum, on new building plans.

Fri., Aug. 3. With Takiychi on fittings for new museum.

Sun., Sept. 16. Our first trip to Marin County over the Richmond-San Rafael bridge. With Mickie, Julian Pitt-Rivers, Christoph von Führer Haimendorf to Muir Woods and a picnic.

Fall semester. Julian Pitt-Rivers and Daryl Ford visiting professors.

Tues., Oct. 2. First session with Bob Ratcliff on our proposed San Luis Road house.

Fri., Oct. 5. Second " " " " " " " "

Fri., Oct. 12. Lunch at Faculty Club: Father Köppers, Joseph Haeckel, Ana Hohenwart guests.

Sat., Oct. 13 Take them to see sights in S.F.

Mon., Oct. 29. E.E. Evans-Pritchard talks to Department Seminar.

Sun., Nov. 11. All day on final chapter of Spanish ms; bibliography also.

Mon., Nov. 12. Sherry Washburn talks to Department Seminar.

Fri., Nov. 16. See Bob Ratcliff on our new house plans.

Tues., Nov. 20. " " " " " " " "

Mon., Nov. 26. Robert Lowie talks on Copenhagen ICAES meetings at Department Seminar.

Tues., Nov. 27. Alfred Kroeber lectures in Dwinelle Hall on the "Natural History of Sex." (This must have been a Jake Gimbel lecture).

Tues., Dec. 4. ditto.

Wed., Dec. 5. Repel artists at Art-Anthropology Committee meeting.

Thurs., Dec. 6. Harry Hoijer evening lecture in Dwinelle Hall. (Topic?).

Fri., Dec. 7. Two hour session with Bob Ratcliff. Then with Mickie, Charlotte Siegel to Los Altos for dinner with Siegels. Also Washburns, Vogts, Spindlers.

Sat., Dec. 8. Three hours with Bob Ratcliff.

Mon., Dec. 10. Julian Pitt-Rivers talks to Departmental Seminar on the Compadrazgo.

Tues., Dec. 11. All P.M. with Gardner Dailey and his architects on the new Art-Anthropology building.

1957

Spring. Julian Pitt-Rivers continues as visiting professor. Pedro Carrasco also a visitor.

Tues., Feb. 12. Mickie starts a museum Philippine exhibit at 2290 Telegraph Ave. Bldg.

Wed. Feb. 27. All A.M. with University architect Takeuchi on new anthropology building plans.

Fri., March 22. Earthquake in Berkeley: 5.5 Richter Scale.

Sun., March 31. I leave PAA Stratocruiser across Pacific to Afghanistan.

Sunday, July 7. Mickie arrives in Beirut from London; I came from Afghanistan on June 30. We return to Berkeley Aug. 24 via Istanbul, Athens, Dubrovnik, Paris, Ottumwa.

Fall semester. Bill Bascom joins faculty as Museum director

Sat., Sept. 21. Robert Lowie dies of cancer. Funeral Sept. 24.

Sat., Oct. 5. Chokecheery Gulch: Melissa bitten by a boxer dog, killed. She to Dr. Teal in Railroad Flat.

Thurs., Oct. 10. Bill Bascom and I meet with Takeuchi all day on new anthropology bldg. Topic: keys.

Tues., Oct. 15. A Faculty Discussion group at our house, 463 Kentucky Ave. Kroeber talks on "What Ethnography is."

Sat., Oct. 19. It is announced that Clark Kerr will succeed Robert Gordon Sproul as President of UC.

Monday, Oct. 28. A.M. meet with Kroeber Hall architect Gardner Dailey and his staff.

Fri., Nov. 1. Little Thinkers (when did I become a member?).

Thurs., Nov. 20. Tenured staff decides to keep Robert Murphy.

Tues., Dec. 24. Glimpse of sputnik at 5:30, back door of 463 Kentucky Ave.

1958

(Tues., Feb. 25, we move into 790 San Luis Road).

Thurs., April 10, Lincoln Constance appoints me Chairman of Anthropology dept.

Mon., May 5. First meeting as new member of Kosmos Club.

Sun., May 11, Dinner at International House for President Sproul.

Thurs., Sept. 25. Meeting with [Kroeber Hall] architects to decide on furniture for new building.

Fri., Sept. 26. All P.M. with architect Fiedler on furniture plans for new building. (temp: 104 degrees in Oakland).

Mon., Sept. 29. Clark Kerr Innaugurated as UC President in Greek Theatre.

Tues., Sept. 30. I have to tell George Pettit the Department does not want him as a permanent staff member.

Sat., Oct. 11. Dinner at Vauxes, McCowns also. Ted insists we go to 790 San Luis Road to see new house, where we find about 25 more friends and colleagues in a surprise birthday party for me.

Tues., Oct. 14. See Chancellor Seaborg about Pettit.

Sun., Oct. 26. Mickie and the Ed Weyers join me on campus for a tour of the new building, not quite completed.

1959

Tues., Feb. 3. All day at the office, first day of registration. Visit new building [under construction, and not yet named], & Hearst Basement, just receiving [equipment?].

Saturday, May 15. Edward Gifford dies.

Wed., June 3. Begin move to new building [i.e. Kroeber Hall, not yet named].

Thurs., June 4. Continue moving.

Fri., June 5. Still moving. My office settled down.

Tues., Nov. 24. Meeting of the Kroeber Hall opening show committee. [First mention in my line-o-day of "Kroeber" Hall].

1960

Monday, Feb. 29. Luncheon at the Women's Faculty Club for the Press, on Kroeber Hall Dedication.

Saturday, March 5. Our big, formal dinner at the Claremont Hotel for the Museum opening show. Mrs. Catherine Hearst, guest of honor. Ceremonies at Kroeber Hall. Drinks at Vice-Chancellor [Jim] Hart's afterward. Drizzle.

Thurs., June 9. Evening at Washburns' moaning about Department conditionss, and Mandelbaum's Chronicle news story about my administration of same..

Fri., June 10. I resign as Dept. chairman. P.M. see Chancellor Seaborg & Bill Fretter.

Tues., June 14. P.M. meeting with the Chancellor (since nothing more appears in my diary I assume I agreed to continue as chairman).

Fri., Oct. 21. Alfred Kroeber Memorial Service in Great Hall at Faculty Club. I deliver the main talk.

1961

Thurs., Jan. 26. Jim LeCron dies. We in San Diego.

Sun., April 9. Lv. Jeremy at Fort Ord to begin military training.

Fri., April 14. Margaret Mead for dinner. Also Kreches, Heizers, Rick and Betty Adams.

Sat., June 11. Jeremy home from basic training.

Applied Anthropology and International Health: Retrospect and Prospect

GEORGE M. FOSTER

George M. Foster is the 1982 recipient of the Malinowski Award, presented at the 42nd Annual Meeting of the Society for Applied Anthropology, Lexington, Kentucky. This annual award is given to a senior colleague in recognition of efforts to understand and serve the needs of the world through social science. The following is Professor Foster's acceptance speech.

IT IS A HIGH HONOR INDEED to be the recipient of the Tenth Malinowski Award, thus to be associated with the names of Bronislaw Malinowski and of my nine distinguished predecessor recipients. When Peter New advised me of my selection, I experienced mixed emotions: first, pleasure and appreciation, and second, concern as to my topic. Fortunately, colleagues came to my rescue; several suggested that I talk about the involvement of anthropologists in international health programs, a field to which I have been exposed for more than 30 years. Hence, the title of this address. Specifically, I propose to note the early events that drew anthropologists to the international health field, to take stock of where we are today, and to suggest the opportunities and problems we may encounter in the future development of the field.¹ To limit my remarks to reasonable length I exclude mental health and family planning, and restrict myself largely to American anthropology.

I use the term "international health programs" to denote those national and international efforts designed to improve the health care services of peoples who, until recent years, have relied largely or entirely on indigenous medical resources. These include most of the people, both urban and rural, in developing countries, and substantial numbers of minority ethnic groups in some industrialized countries. The common characteristic of these programs is that they are cross-cultural: historically, health care providers and health care recipients have represented distinct cultural, social or ethnic groups. Whatever the setting, the basic problems associated with planning and delivery of health care, and utilization of services, are essentially the same.

A generation ago physicians and anthropologists alike defined the problem of providing better health care for traditional peoples as how to persuade them to accept scientific medicine. As anthropologists, our fundamental research task was to learn those things about the health beliefs and practices of these peoples that would enable health care providers to convince them of the superiority of modern medicine over their traditional therapies. Our practical goal was to further effective cross-cultural communication which, we assumed, would lead to scientifically acceptable health beliefs and behavior.

Today the goals of international health programs are more

realistic, namely to determine and develop appropriate forms of health care that will meet the diverse needs and expectations of the world's peoples. While it is still assumed in medical circles that scientific medicine will be the cornerstone of such programs, it is now respectable—even fashionable—to voice opinions no one would have dared utter a generation ago. Particularly in the context of primary health care programs, the roles of traditional therapists and therapies are being seriously discussed and, in one medical subspecialty—midwifery—major use is made of traditional birth attendants in many national programs. Almost without exception, World Health Organization and Agency for International Development reports stress the need to understand the sociocultural aspects of health and illness behavior, to learn about the dynamics of "community participation" in health care programs, and to incorporate behavioral scientists into health research projects. On the surface, at least, it looks as if the time is propitious for anthropologists to play an increasingly important role in international health programs. Time will tell whether this will prove to be the case.

Some Origins of International Health Programs

International health programs long antedate applied anthropology. By the middle of the 19th century European and American medical missionaries had begun to cater to the needs of body and soul of non-Western peoples. Most missionary work has been curative rather than preventive. In the beginning this failure to include preventive measures may have reflected the embryonic status of public health in the last century. Except for smallpox vaccination, effective means to prevent most illnesses were not developed until about three generations ago. But it is also tempting to speculate that just as it is more satisfying to "save" a repentant sinner than to prevent someone from falling into sin, so is it more gratifying to save a seriously sick patient than to prevent that patient from falling ill.

At a government-to-government level, international health concerns were first reflected on a major scale in the International Sanitary Conferences, the first of which was held in Paris in 1851. In contrast to missionary activities, the concern of European governments—the principal organizers of the

Conferences—was preventive, to control the cholera epidemics which, beginning early in the 19th century, periodically threatened European port cities (Howard-Jones 1975). In the Americas the major international health threat was yellow fever. It was the desire to control this disease that led to the establishment of the first continuous international health organization, the International Sanitary Bureau, later the Pan American Sanitary Bureau, in Washington, in 1902 (Howard-Jones 1980). Prevention by means of environmental sanitation was the rationale that underlaid the United States government's successful effort to eliminate yellow fever in Havana in 1901, and shortly thereafter in Panama, Rio de Janeiro, and other tropical port cities. Later the Rockefeller Foundation joined in international yellow fever and hookworm control efforts. Needless to say, anthropologists played no role in these programs, although, as a historical oddity, one may note the 1921 article by Philip Ainsworth Means on "Sociological Background of Sanitation Work in Peru" in the *American Journal of Public Health* (Means 1921).

Nutrition Programs

The earliest role of anthropologists in the international health field appears to have been a function of colonial concern in the 1930s and 1940s with indigenous diet and health. By the mid-1930s both anthropologists and administrators in Africa realized that under the impact of wage labor and migration, indigenous diets had deteriorated while, at the same time, hard and continuous mine and plantation labor required a level of nutrition not always realized under preexisting conditions. As early as 1934 Sir Raymond Firth wrote on "The Sociological Study of Native Diet," suggesting "a possible systematic field approach to the situation" (Firth 1934:402). In 1935 the International Institute of African Languages and Cultures established an interdisciplinary Diet Committee, whose members included Sir Raymond and Audrey Richards. The latter's *Land, Labour and Diet in Northern Rhodesia* (A. Richards 1939) was one outcome of the work of the committee. Although it is hardly a guide to improving indigenous diets, it was written in the context of developmental health problems and, in the author's words, had a "practical bent." Concern with dietary problems is shown in the work of other British anthropologists of the period, including Bronislaw Malinowski, who devotes an entire chapter to "Problems of Native Diet in the Economic Setting" in *The Dynamics of Culture Change* (Malinowski 1945).

In the United States the National Research Council set up a Committee on Food Habits in 1940; its initial members included Ruth Benedict, John Cooper, and John Province. Shortly thereafter, Carl Guthe joined as chairman and Margaret Mead as executive secretary. Although the immediate goal of the committee was to provide guidance on how best to nourish an America at war, the applied and international implications for nutrition policy were early recognized, particularly the problem of meeting food shortages in other countries after the war. The introduction to the committee's 1945 *Manual for the Study of Food Habits* makes this clear: "The study of food habits is an applied science to which many different sciences have to contribute in order to find answers to the questions that come up continually in our attempts to improve the nutri-

tion and living habits of the peoples of the world" (National Research Council 1945:13). The applied science of food habits was seen, not as a static description of what people eat, but as a dynamic problem the goal of which was to learn how actively to intervene to change food habits in directions believed to promote better nutrition (National Research Council 1943:20). Although the committee sponsored no major cross-cultural research, and hence cannot properly be said to have been involved in international health, it made explicit the central theme of contemporary anthropological participation in international health programs: the process of directed culture change.

Oddly, this promising early collaboration between nutrition specialists and British and American anthropologists had little influence on succeeding developments involving anthropologists in international health programs. Writing in 1964, Margaret Mead said that after the war, the newly-established international organizations concerned with health—WHO, UNICEF and FAO—made only minor use of what anthropologists had learned about the problems of changing food habits (Mead 1964:4).

The Navaho Laboratory

Health research and medical practice among the Navaho Indians played a much more important role than nutrition studies in clarifying problem conceptualization and in providing research guidelines for the budding medical anthropologists of the postwar period. Alexander and Dorothea Leighton began their research in 1940, and in 1944 published *The Navaho Door* (Leighton and Leighton 1944). This was the first major study to show in detail, and with abundant examples, the medical and behavioral problems inherent in the cross-cultural practice of medicine. This book profoundly influenced those of us who began research on international health programs in the early 1950s, as did Alice Joseph's pioneering study of White physicians and Navaho patients (Joseph 1942), the first to deal with how the cross-cultural practice of medicine affects the physician.

The Navajo-Cornell Field Health Project at Many Farms, Arizona, begun in 1955, went beyond these earlier studies in that it combined research and medical intervention to provide culturally more appropriate health care services (Adair and Deuschle 1958; McDermott et al. 1960; C. Richards 1960). Particularly innovative was the creation of the role of "health visitor," a Navaho paramedic and health educator who may be viewed as the prototype of the "village health worker," the mainstay in the delivery of primary health care in developing countries today (Adair 1960).

Early Institutional Ties

Significant anthropological participation in international health work requires a formal tie to an organization whose primary responsibilities are in the international (or cross-cultural) field. It was not until such organizations—or more precisely, individuals in such organizations—began to hire anthropologists that the base was established for anthropology's involvement in international health activities. It is from the

first use of anthropologists in institutional contexts such as these that we must date this branch of medical anthropology. I believe Cora DuBois was the first American anthropologist to be hired by an international health organization. She joined the World Health Organization in 1950 for about a year. "I saw my role," she has written me, "[as that] of an observer and consultant." In other words, she did not plan to recommend specific courses of action. DuBois's assignment was plagued by the problem that has afflicted many subsequent anthropological consultants: neither she, nor the hiring organization, really knew what she should do. Shortly after joining WHO she left for India and Southeast Asia where, in the regional office, her reception was unenthusiastic. "I was left to make my own plans and schedules and I was more than a little perplexed as to what was expected of me. . . . Much the same perplexity about my role obtained when I returned to Geneva" (communicated by C. DuBois).

Three separate and unrelated events that proved to be more propitious for anthropologists in international health took place shortly thereafter. Edward Wellin was hired by the Rockefeller Foundation to work in Peru, Benjamin Paul was hired by the Harvard School of Public Health, and Richard Adams, Charles Erasmus, George Foster, Isabel Kelly, Ozzie Simmons, and Kalervo Oberg, all of the Smithsonian Institution's Institute of Social Anthropology (ISA) began their Latin American research for the Institute of Inter-American Affairs (IIAA). These developments came about as follows:

John L. Hydrick, an American public health physician, had spent some years in the Netherlands East Indies in the 1930s engaged in health education and rural hygiene work. He wrote what is probably the first "how-to-do-it" manual describing how the members of a traditional community should be approached by health workers, and how their interest in changing traditional health behavior might be elicited (Hydrick 1942). In 1950 he was working under Rockefeller auspices in Peru, where he found many of the same problems encountered in the Indies. At his request the foundation hired Edward Wellin, then a graduate student at Harvard. Wellin arrived in Peru in late 1950 and remained until 1954. His classic article on water boiling in the Paul casebook is one of the products of this Peruvian work (Wellin 1955; also 1953a, 1953b, 1958).

The antecedents of the Paul appointment are more complex. In 1930 Esther Lucile Brown, a recent Ph.D. in anthropology at Yale, signed on with the Russell Sage Foundation for a six-month assignment, an appointment that was to continue for more than 30 years. Foundation concerns at that time dealt with the professions, and it was this interest that led to Brown's pioneering research on nursing (Brown 1936). In 1948 Donald Young, a distinguished sociologist, became president of the Russell Sage Foundation and launched a new applied program whose goal was to determine what anthropology, sociology, and social psychology could offer to and learn from "social process" programs. Because of her experience in the field, he asked Brown to begin with health. Her assignment was to find out if schools of public health, nursing, and medicine would be interested in having behavioral scientists on their faculties. If such schools could be found, the foundation would help them locate appropriate scholars and initially pay their salaries with the understanding that the institutions gradually assume financial responsibility. The University of Colorado's School of Medicine was the first to take advantage

of this offer, with the appointment of Lyle Saunders, a sociologist, to the Department of Preventive Medicine and Public Health. The outcome of this appointment was *Cultural Difference and Medical Care* (Saunders 1954), a book that describes the problems inherent in the cross-cultural practice of medicine and recommends action to bridge the "cultural chasm" separating Anglo health care providers and Spanish American health care recipients (communicated by E. L. Brown).

Paul's appointment was made possible by the same program. In the spring of 1951 he was approached by Hugh Leavell, Professor of Public Health Practice at the Harvard School of Public Health who, with Russell Sage support, asked him to join his faculty to introduce social science concepts and methods to graduate students. Paul's association with the Harvard School of Public Health lasted from 1951 until 1962. The major product of this collaboration was, of course, *Health, Culture and Community* (Paul 1955), the single most widely read and influential book in all of medical anthropology. Although Paul did not see his appointment as specifically international, his own research experience in Guatemala, and the large number of foreign students at the Harvard School of Public Health, ensured that major emphasis would be placed on this aspect of health, as is evidenced by the fact that 12 of the 16 cases in *Health, Culture and Community* describe programs and conditions in foreign countries (communicated by B. D. Paul).

The entry of Institute of Social Anthropology anthropologists into international health came about in still another fashion. The institute, which had been established by Julian Steward in 1942 as a part of the United States government's Latin American cultural relations policy, was financed by the Department of State. Steward's justification for the institute was its potential applied value in training Latin American social scientists and providing sociocultural information for Latin American governments to use in their postwar developmental programs. Steward saw the roles of his staff members as teaching, conducting research with Latin American colleagues and students, and publication. But, in keeping with prevailing views about applied anthropology, he did not envisage the participation of staff members in specific development programs, nor were they to make policy recommendations.

I joined the ISA in 1943, spent two years in Mexico, and returned to Washington in 1946 to succeed Steward as institute director. Initially I left unchanged his policies and philosophy. My principal teachers—Herskovits, Kroeber, and Lowie—had successfully inoculated me against the germ of applied work and I was content to continue in a traditional course. But by 1950 the U.S. government had largely lost interest in Latin America in favor of new concerns in Africa and Asia. I was advised that 1951 would be the last year the institute would enjoy State Department funding. This news constituted my first lesson in directed change: nothing encourages one to re-examine fundamental premises more rapidly than the threat of economic disaster. So, after consulting my colleagues in Latin America, in January 1951 I approached officials of the Institute of Inter-American Affairs. The IIAA, the first American technical aid program of modern type, and the forerunner of AID, was established by Nelson Rockefeller in 1942 to carry out bilateral developmental projects in Latin American coun-

tries in health, agriculture, and education. I proposed that we make analyses of some of the social and cultural aspects of the institute's health programs, especially of the barriers being encountered in introducing preventive medicine in traditional communities. Permission received, in the spring of 1951 Kelly, Erasmus, Simmons, and Oberg carried out analyses of eight health centers in Mexico, Colombia, Peru, and Brazil. In Guatemala, Adams simultaneously began research that led to "A Nutritional Research Program in Guatemala" in the Paul case book (Adams 1955a).

The findings of this research were issued in mimeographed form in July 1951 (Foster, ed. 1951), and they aroused a good deal of interest among professional public health personnel, including Henry van Zile Hyde, chief of the IIAA Health Division. Recognizing the potential contribution of anthropology to the solution of the problems his division was encountering, he asked ISA staff members to participate in a United States Public Health Service evaluation then being planned of the first ten years of IIAA bilateral health programs in Latin America. This evaluation, with the participation of Erasmus, Foster, Kelly, Oberg, and Simmons, was carried out from October 1951 to March 1952. As part of the agreement, the IIAA also financed the ISA during all of 1952, thus solving our budgetary worries for that year. The IIAA found the anthropological contributions to its programs to be so useful that it asked the ISA to become one of its divisions, a move that would have entailed leaving the Smithsonian Institution. This action was not taken, but Erasmus, Kelly, Oberg, and Simmons transferred to the IIAA as direct-hire U.S. government employees where they continued in developmental research for periods of from two to ten years. Some of the early seminal papers on anthropological aspects of international health programs resulted from these associations (e.g., Erasmus 1952, 1954; Foster 1952, 1953a, 1953b; Kelly 1955, 1957; García Manzanedo and Kelly 1955; Oberg and Rios 1955; Simmons 1955a, 1955b, 1957; Jenny and Simmons 1954).

After the Institute of Social Anthropology program in Guatemala was terminated in mid-1951, Adams continued health research, first on a State Department grant under the auspices of the Instituto de Nutrición de Centro-América y Panamá (INCAP), and from 1953 to 1956 as a Pan American Sanitary Bureau staff member. Several pioneering publications came from this work (Adams 1951, 1953, 1955a, 1955b). Nancie Gonzalez's association with INCAP began in 1955, and during the following decade her Guatemalan research contributed significantly to our understanding of international health problems (Solien and Scrimshaw 1957; Solien de Gonzalez 1963a, 1963b). Also noteworthy were the extensive medical research and intervention programs among Mexico's Indian groups carried out in the 1950s by that country's National Indian Institute under the direction of Gonzalo Aguirre Beltrán (Aguirre Beltrán and Pozas A. 1954; Aguirre Beltrán 1955), who, you will recall, delivered the first Malinowski lecture nine years ago.

In comparing the disappointments of DuBois's assignment with the positive results of the work of those who shortly followed her, a critical difference in the institutional setting stands out. No one in WHO was interested in DuBois's work. In contrast, the work of Wellin, Paul, the ISA anthropologists, Adams, and Gonzalez was successful in large part because of the interest and support of strategically placed culturally sen-

sitive public health physicians: Hydrick in Peru, Leavell at Harvard, and Hyde in the IIAA. And without the support of Nevin Scrimshaw, medical doctor and nutritionist, then director of INCAP, the work of Adams and Gonzalez in all likelihood would not have prospered.

Anthropologists in International Health Programs: The Current Picture

Many, if not all, of the details of the involvement of American anthropologists in international health programs since the 1950s are described in a series of reviews of medical anthropology, especially Caudill (1953), Polgar (1962), Scotch (1963), Fabrega (1972), Lieban (1973), and Colson and Selby (1974). In some ways it is an impressive story. At the same time, the institutionalization of anthropology in international health organizations—a development I would confidently have predicted 30 years ago—has not taken place. Although medical anthropology increasingly is institutionalized in schools of public health, nursing, psychiatry, and even medicine, most international work continues to be done on the same ad hoc basis as in the 1950s, and assignments depend on the personal networks that link anthropologists and health personnel. In considerable part the failure to institutionalize our international health roles must be laid at our own doorstep. In the 1950s we were enthusiastically welcomed as colleagues by health policy makers and planners; we enjoyed their respect and confidence, and they were anxious to engage our services.

In part because of the low status accorded applied anthropology—and international health work was viewed as "applied"—and in part because of the abundance of openings in American universities, anthropologists largely rejected the proffered hand of our public health colleagues. We accepted academic rather than applied appointments. In retrospect, we muffed our greatest applied opportunity. With the interest and support of foundations, and government and international agencies, we had the opportunity to create a developmental science that might significantly have altered the pattern of international aid programs. Insofar as international health programs are concerned, we have made progress since the 1950s, but less than we should have.

To the best of my knowledge there is no inventory of anthropologists who work or have worked in international health programs. One thing seems certain, however. Whereas in the 1950s international and cross-cultural health activities constituted the largest part of medical anthropology, today these activities are but a small part of the field. Paradoxically, at a time when international health work attracts anthropologists, the national and international organizations that are our obvious allies and employers show little interest in us. The situation of 25 years ago is completely reversed.

Among the major organizations employing anthropologists to work on health problems, AID continues, as in the past, to lead the field. Four "direct-hire" anthropologists are based in Washington, and at least two more are members of field missions. A much larger, fluctuating number work under long- and short-term contractual arrangements, while a few serve as "consultants" for not to exceed six months a year. Still others are employed indirectly via university, Population Council,

American Public Health Association, and private consulting firm contracts (communicated by Barbara Pillsbury).

In contrast, the World Health Organization makes relatively little use of anthropologists. At headquarters in Geneva there is but one full-time anthropologist, and he is scheduled to complete his assignment this year. Anthropologists serve, however, as short-term consultants, as "expert committee" and "expert panel" members for specific projects, and they participate in workshops dealing with specific health problems. The regional offices of WHO also make fairly limited use of anthropologists, largely in the capacity of consultants. None, I believe, has a full-time staff member. The World Bank has assigned an anthropologist to problems of population and nutrition and has sponsored contract research on appropriate technologies for water supply systems. UNICEF appears to have made the least use of anthropologists of all the major organizations. This may change, however. "Given UNICEF's mandate for children and women, the future of women anthropologists (who tend to include women, or gender issues in their research concerns) looms more favorably in UNICEF than that of men" (communicated by Mary R. Hollnsteiner).

Apart from AID, then, it appears that international health organizations largely view the role of anthropologists as that of short consultants for specific projects, as committee members, and as workshop participants. Less frequently, they participate in teaching programs, in the evaluation of health behavioral science resources in Third World countries, and in organizing meetings to consider behavioral science research on health problems. It is on tenuous bases such as these that we must build if we wish to play a more important role in international health programs.

International Health Personnel: What They Want of Anthropology

I now turn to the kinds of help international health personnel would like to have from anthropologists, to what I think we can in fact offer them, and to the problems that make it difficult to realize the hopes of both groups. I draw largely on my experiences in recent years as WHO consultant in South and Southeast Asian countries. Since 1975, WHO's primary strategy in providing health care and disease control has been through primary health care, an approach in which, at least in theory, the community plays a major role in defining health priorities, marshalling resources, and providing services. The primary health care philosophy is, of course, a restatement specific to health of the community development movement of the 1950s, and programs are encountering many of the same problems that surfaced a generation ago, particularly obtaining community participation. The principal programs where community participation is deemed essential include the control of a group of tropical diseases (e.g., malaria, schistosomiasis, filariasis, leprosy, yellow fever, and infant diarrheal diseases), immunization programs against infectious childhood diseases, and installation and maintenance of water supply systems and latrines.

Not surprisingly, then, the question I am most frequently asked is: "How can we elicit community participation in our programs?" Just as among community development workers a generation ago, so today among health personnel there is a

hopeful assumption that there is a right "key" which, if only anthropologists can discover it, will unlock the door to whole-hearted community cooperation in primary health care activities. It is difficult to convince WHO personnel that the problem is not one of hitting upon a clear way to communicate ideas, to motivate community members to action, but that there are basic structural and historical factors having to do with economics, caste and class, religion, politics, land tenure, and the like that frequently militate against genuine community cooperative efforts in solving health problems.

A second question has to do with "early case finding" and "case holding." With leprosy, for example, early identification of symptoms usually makes possible rendering the patient noncontagious in as little as 24 hours, and regular treatment prevents disfigurement. Yet leprosy patients are reluctant to be so identified, and usually they come to the attention of physicians only after a long period in which they have threatened the health of others, and when disfigurement can no longer be concealed. Even with early identification, treatment must continue for many years, sometimes throughout the patient's life. Here is the problem of case holding: in the absence of symptoms, people who feel well see no need to continue with a treatment which, in all likelihood, they do not understand. Early case finding and case holding are equally important problems in the control of tuberculosis and other chronic infectious diseases. Case holding is also a major problem in immunization programs: in the usual series of three visits to the health center there is a rapid drop-off after the first and second injections, so that many children do not receive complete protection. How can parents be persuaded not only to begin immunizations, but to complete the prescribed series? That is the question we are asked. Other common questions have to do with the difficulties encountered in persuading people to build and use latrines, and to cooperate in the maintenance of community water supply systems.

The striking thing about these questions is that almost all assume that effective health care can be achieved only when members of traditional communities change their health behavior. Rarely if ever is the question asked, "How can anthropologists help to change bureaucratic behavior that inhibits the design and operation of the best possible health care systems?"

Anthropologists: What We Can Offer International Health Programs

In trying to answer this question, we must distinguish between what we, as anthropologists, would *like* to do, and what realistically we can *hope* to achieve. For example, we would *like* to study health bureaucracies, attempt to determine ways in which their structure and operations might be changed to offer more effective services, and communicate to health personnel the urgency of making these changes. Most—but not all—of the health personnel with whom I have worked in recent years are genuinely surprised when it is suggested that changes in their beliefs and behavior are just as critical as changes in community members' beliefs and behavior, if adequate health care is to be provided. And those who do recognize the importance of bureaucratic factors in health care delivery feel—realistically, I suspect—that this is a constant

about which little can be done. If changing behavior will result in effective primary health care, it must be community, not bureaucratic behavior, that changes.

A more hopeful area in which we as anthropologists can contribute to health care programs is health research. What international health planners want, reasonably enough, is operational research, research whose results can be incorporated into program planning and operations. As a step in this direction, in the fall of 1981 the Southeast Asian Regional Office (SEARO) of WHO hosted a six-country workshop in New Delhi entitled "Appropriate Technologies for Behavioural Science Research on Health Problems." Workshop participants, who included anthropologists, sociologists, nutritionists, medical doctors, and health educators, concluded that a principal reason why so little health research in their countries has been operational is the near-exclusive reliance on survey techniques. Survey research is emphasized in part because some researchers feel it is more "scientific" than observational techniques, and in part because medical doctors prefer it. The latter are so imbued with the biomedical research model that qualitative information on human behavior is suspect to them.

In spite of these problems, workshop participants felt that it is realistic to anticipate much improved behavioral science research on health problems in their countries. They concluded that a "comparative case study" design combining participant-observation with structured and unstructured interviews is most apt to provide the needed information.

A third important opportunity for anthropologists in international health programs is to provide convincing evidence that primary health care must take into consideration the prevailing health beliefs and practices of the population to be served. It is anomalous indeed that although WHO has a major program in traditional medicine (Bannerman 1977), and WHO staff members continually speak of the importance of incorporating traditional therapies and healers into primary health care programs, almost no one thinks about how to research ethnomedicine and, after the results are in, to apply these findings to ongoing programs. Nevertheless I am optimistic about progress in this area. With patience, and with good examples, I believe we can put across the idea that effective health care must reflect knowledge of what community members believe and do.

The Problem of Theory

I now turn to a more general question, namely, What does anthropology gain from research on international health programs? Like most new fields, medical anthropology, including international health, has been charged with failure to develop theory. Twenty years ago Caudill, in commenting on Polgar's survey, "Health and Human Behavior" (Polgar 1962), noted that anthropologists had "done a considerable amount of careful descriptive reporting" but that "we have not, as yet, made the conceptual and theoretical contributions to this field of which we are capable" (Caudill 1962:181). In the same commentary, Kundstadter noted that the literature reviewed by Polgar "is more impressive for its bulk than for its importance in relation to general theories of human behavior. It is a pity that a field with such apparent practical significance has re-

ceived so little theoretical consideration" (Kundstadter 1962:185).

It is probably the fate of every new anthropological field to be judged initially as showing limited theoretical potential. In reviewing literature on peasants in the same year, Geertz, while noting its "undoubted descriptive value," felt that "it is not, at the moment, a very likely place to look for ideas, and certainly not for systems of ideas" (Geertz 1962:18). Yet by now it is clear that like peasant studies, medical anthropological studies have made important contributions to theory. This is because, as Scotch pointed out long ago, "what comes out of research is not dependent on the *nature* of the problem to be studied but rather on the *way* the problem is studied" (Scotch 1963:32). The concern of the anthropologist is to study human behavior. *All* human behavior is grist for our mill, and all good research data—whatever the context in which they are gathered—have theoretical potential. The study of a health clinic in Thailand is potentially as productive of theory as is the study of a hill tribe or peasant village in the same country.

In speaking of theoretical contributions, it is important to distinguish between two levels: (1) general anthropological theory, and (2) theory specific to medical anthropology and to international health.

With respect to general theory, the main contribution to emerge from international health research is in the area of sociocultural change, and particularly the factors that promote and discourage change. I believe a large part of current anthropological theory about the dynamics of culture change has come from the applied field: work in agriculture, community development, and health programs. A particularly important theoretical point emerges from the growing literature on the "hierarchy of resort" in seeking medical care (Schwartz 1969), the study of the sociopsychological forces that lead patients to choose between alternative therapies and therapists. A general theory to explain how people make up their minds when faced with viable choices will, of course, go well beyond the boundaries of anthropology itself.

International health research has also contributed to our understanding of the relationship between belief and behavior. Behavior, it is often assumed, is a function of belief. Logically, then, a change in behavior must be preceded by a change in belief. At least until recently this has been the rationale for health education—that improved health habits will follow educational efforts that change traditional beliefs about health and illness. Yet it is now clear that health behavior is not so much a variable dependent on belief as dependent on the perception of advantage that comes from changing behavior. Thirty years ago, Erasmus found among postparturient women in a hospital in Quito that their traditional birth beliefs remained intact while they happily conformed to a maternity ward regimen that, in many respects, ran counter to traditional practices. Mothers found they regained their strength more rapidly in the hospital than at home, and they felt that their infants had a better start on life than when delivered by a midwife (Erasmus 1952). Subsequent health research has confirmed Erasmus's conclusions: people are pragmatic, and will adopt new behavior forms if they perceive advantage in so doing. Conversely, we now know, the acquisition of new knowledge will not automatically lead to changes in behavior; individual priorities and personal preferences override strict rationality.

Still another point on which, I believe, international health data have contributed to general change theory is that of the importance of social costs. Planners often assume that economic costs are the basic determinants of change. These are important, but international health research reveals that social costs may also be very important. A comprehensive theory of change must take into account both economic and social costs to the individual and the community.

With respect to comprehensive theory specific to medical anthropology and international health, I am less certain about what we may expect. The field of medical anthropology is so broad, and so diverse, and has ties to so many biological and social disciplines, that it seems unreasonable to expect any general theory applicable to all of medical anthropology. But I believe we already have a very substantial body of "middle-range" theory that is specific to human behavior in health contexts. I take a reasonably good explanation of a particular form of behavior to be a theoretical statement. By "reasonably good" I mean an explanation that has general, if not absolute, predictive value. In this sense we have good theories about the behavior of leprosy patients: they tend to avoid having their cases diagnosed because they fear ostracism. We have theories that predict the probable sequence of events when antimalarial DDT spray teams enter a community. And we can explain why people in traditional communities often are uninterested in building latrines, and why, once built, they may not be used. We can predict, in the international health field, that preventive medicine will be more easily introduced into a traditional community if the curative "felt needs" of the population are already being met. Many of these middle-range theories, I am confident, will with the passage of time become incorporated into more general theories.

Prospects for the Future

In 1961 Louis Miniclier, then head of the Community Development Division of AID, whose office employed more anthropologists than any other branch of government, noted three problems he had encountered in using anthropologists: defining their roles, producing evidence demonstrating to administrators that anthropologists contribute to foreign aid objectives, and finding anthropologists willing to serve overseas for two or more years at a stretch (Miniclier 1964:187). These are the problems that bedevil us today. Satisfactory long-term roles for anthropologists in international health programs are almost as elusive today as when Cora DuBois struggled to decide what she should do. Short-term assignments as consultants, service on international health committees, and even research projects of a year or two are, of course, "roles," but they are not institutionalized roles understood by others in medical hierarchies, in the same way that the roles of doctors, nurses, and health planners are understood. We can continue on the same ad hoc basis that has marked most international medical anthropological work during past decades, but until we fill institutionally recognized roles our work will be marked by improvisation. To work effectively we still need medical "godfathers"—a Hydrick, Leavell, Hyde, or Scrimshaw—sympathetic to anthropologists and knowledgeable as to how to use them. But the age of our godfathers appears to have passed.

To those of us gathered here tonight, anthropologists have abundantly demonstrated their ability to contribute to international health programs. But it is well to remember that what we regard as convincing evidence is not so regarded by all members of the medical profession: after all, our evidence is not based on hard data. It is not enough to do sound research on health problems, and it is not enough to make specific recommendations for recognized problems. We have a job of selling, of selling ourselves and our skills to those who determine international health policies and who decide who will be employed to carry out these policies.

It is only with respect to Miniclier's third point—the need for long-term commitment by anthropologists—that the picture has improved significantly. It is now acceptable to aspire to a nonacademic, goal-oriented career. With the medical anthropological training now offered in a number of universities, we are preparing anthropologists with the specific skills needed for international health work; and many of them feel a long-term commitment to the international field to be a desirable choice. It is up to us who, in any capacity, are involved with international health programs to do our best to create the institutionalized roles that will enable them to realize their aspirations.

NOTES

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LETTERS TO THE FIELD: REFLECTIONS ON TWO
DECADES OF CORRESPONDENCE FROM GEORGE FOSTER

Cynthia Nelson

As I reflect upon the influence that George M. Foster has had (and continues to have) on my personal and professional career in anthropology, I find it most appropriate and natural to concentrate on his own letters to me during the past two decades. Appropriate because it was a letter to me from George that influenced my decision to enter the department of Anthropology at Berkeley in the fall of 1958; and natural because ever since I have known George, the letter has continued to be the medium of much of our communication especially including the fieldwork years in Mexico and my "long-term" sojourn to Egypt.

Twenty-one years ago when my intellectual searches finally led me to the discovery of anthropology, I was in a dilemma regarding where to pursue my doctoral studies. Like many aspiring graduate students in the late fifties I had applied and been accepted to several universities. The gnawing apprehension that this "choice" was to be a crucial life commitment was exacerbated by the cold and formalistic answers I was receiving to my personal letters inquiring about housing, living costs and the general socio-cultural "setting" within which I was to embark on my studies. One day a letter arrived from the Graduate Advisor of the Department of Anthropology at Berkeley. Not only did he address every question that I had raised but he had taken the time to send me several clippings advertising available apartments for rent; to outline the basic kinds of living costs facing a new resident to Berkeley; and to warmly welcome me to the department, suggesting that upon arrival I stop in and see him about possible research assistant activities in the department. That Graduate Advisor was George M. Foster and that letter launched me into one of the most exciting intellectual milieux where anthropology was being taught and fought. It also inaugurated a friendship that has grown and transformed over the years.

What began as the usual student respect and admiration for a professor whose national and international reputation was well known, developed into a craftsman-apprentice relationship in the best tradition of "learning the art" at the side of the master. He was one of the few professors at that time who personally arranged to have his students experience the joys and ardors of fieldwork prior to their longer sojourns to the field. He instilled in his apprentices a respect for and appreciation of the dignity and humanity of our hosts while training us in the painstaking detail of fieldwork. All the while encouraging his students to pursue their own ideas and intuitions. I personally credit him for these values and skills becoming part of my own basic philosophy and repertoire in anthropology. To illustrate the quality of that influence I should like to quote from his monthly letters to me during the period I was undertaking my fieldwork in Erongaricuaro, Michoacan 1960/61. The fact that I have kept these letters over the years suggests the degree to which I still find them meaningful and insightful on a number of levels. For example here are some of his personal views on the experience of field work:

I'm glad fieldwork continues to please. It's the supreme experience of anthropology, something no other discipline can duplicate. If science ever eliminates the traditional field year, anthropology will be the poorer for it. It's something that can't be hurried, and everyone has to live it out for himself (*Letter*, December 6, 1960).

I'm, needless to say, delighted that you feel you haven't made a mistake, and that you find you have learned so much about the field experience I think, too, it is something you find wherever chance sends you, although I have always been glad chance sent me to Mexico rather than to a Chinese enclave in San Francisco or something of the sort. You are having the normal experience in seeing that the field is not all glamor, that it is hard work, at times boring and at times makes one want to scream (*Letter*, February 11, 1961).

I think primate behavior is interesting and valuable, but I don't think primates will replace humans for the social anthropologists who want to understand society, and I think you have to be plunked in the middle of a vigorous society, to be able to understand what it's all about. There are some experiences, such as sex, psychiatry and a close death in the family, that can't be understood vicariously. They have to be experienced. The field experience, as you are now getting it, is one of these things. Hemingway adds the bull fight to the list of things that have to be experienced, and I concur (*Letter*, February 11, 1961).

Good fieldwork is as much art as science, and if you feel less systematic than you think you should be, you more than compensate for your ability to enter into and feel the community. Indeed it is a rich and wonderful and rewarding experience and one that lots of people never achieve (*Letter*, May 5, 1961).

Not only did he consciously and continuously express enthusiasm for the field experience but also his letters were a constant source of insight as to what to be "on the lookout for." He was prodding, perceptive and unyielding when it came to being tenacious in pursuing accurate data. For example, in reacting to my fieldnotes, he wrote:

I was interested in that your one case shows this basic loneliness, which seems to me to be characteristic of so many village Mexicans. Also, A's reason for not getting married—paternal opposition is common in Tzintzuntzan. Again, her emphasis on wanting to work alone is in line with my experience—people don't like group activities. . . Are your birth data your own count or those of an assistant? Tzintzuntzan for last year had practically as many as you have, yet it is appreciably smaller. If the count is not yours maybe you'd better go over the books yourself. If the data are accurate, Erongaricuaro has a much lower birth rate than Tzintzuntzan and that doesn't make sense (*Letter*, January 16, 1961).

Your notes on the use of banks interesting. This is a very important point, since it seems to me when people begin turning to banks they are on the threshold of great changes in use of capital. It opens the way for investment in a number of things. In Tzintzuntzan money goes into land, and since land is limited, prices go to fantastic heights. The system has to break and you may be in the right place at the right time to find out how it breaks—maybe setting oneself up with a couple of looms is one way out. Of course, bees and chicken afford another (*Letter*, November 7, 1960).

Your compadrazgo data are good and seem to fit my pattern. Try to get lots more on this, since the subject is still wide open as far as it really works. With adequate data you can have a good supplemental article. And, speaking of this, in doing fieldwork it is always wise to bear in mind topics that will make good individual articles, apart from the dissertation, and to make sure data are full enough for this extra treatment (*Letter*, December 9, 1960).

Perhaps what was most valuable to the neophyte fieldworker was George's generosity in sharing his theoretical ideas with his students as he was working them through—even those of us who were thousands of miles away in the field. I remember particularly his letters to me when he was developing the idea of the dyadic contract as an explanatory model of peasant village social structure.

I have been having fun working out a paper for the AAA meetings in Minneapolis the end of next week. "Dyadic reciprocity as a cohesive factor in Tzintzuntzan society." As I have worked over the data I have more and more been impressed by what seems to me to be the elemental basis for social organization. Whatever the institution—family, compadrazgo, friendship, church, etc.—in fact life is organized on a series of *paired contracts* that ego makes, all through life, with the people whom he (or she) works most closely. . . . These ties are expressed and given substance by a continuing exchange of tangible goods and services. As long as the exchanges continue, the contract is in force. When they end, even though the institutional tie—compadre, brother, next door neighbor—continues, the relationship is dead. . . . I find I see things in Tzintzuntzan when I think of society in this way that I didn't see earlier. For one thing, it helps understand why cooperative work is such a problem—the structure of society, dyadic, just doesn't produce the small groups equally bound together that could be the basis for cooperative work. I started on this line of thought when I began to realize how important it is to offer visitors a bit of cooked food—not all visitors, but just some of them—who turned out to be the dyadic partners in these informal relationships. I am running off a copy of the paper which I shall send under separate cover (*Letter*, November 7, 1960).

I have rewritten the paper I sent you beyond recognition. Little by little it dawned on me that the exchange pattern is not the really significant thing, that this merely validates the important thing, which is that all life is organized (outside the nuclear family) in terms of informal, implicit dyadic contracts, which include people within the family, people in the neighborhood, compadres, relatives, *but not all of the people in any of these categories*, who, in traditional role analysis, all have the same position vis-a-vis ego. It develops into a wonderfully simple model that makes me ask all sorts of questions, the data in a whole series of fields fall into line. For example, what is that vague thing, *personalismo*, except a situation in which two people figure they can get what they want more easily by ignoring the theoretical roles of the system in which they meet? Why won't Mexican villagers cooperate? No two people have the same group of partners so there is no common ground to start people out. On the other hand, the lack of blocks works in another way: we get lots of feuds—which almost by definition are between individuals—but few if any real factions, in the Indian sense. To have factions you have to have functional extended families, unilineal descent groups, or other units that define common positive and negative interests. Sorry I can't go into details, but I hopefully am on to a model for bilateral kinship societies that will be helpful in a whole lot of ways (*Letter*, January 16, 1961).

These letters highlight the development of some of Foster's most enduring theoretical contributions to peasant social organization as well as illustrate those qualities of honesty, integrity and forthrightness—qualities which he has not relinquished over the years. His openness to intellectual dialogue with his students created a sense among us at Berkeley that Foster took us seriously; for he made us feel that we were as much a part of his own intellectual growth and development as he was to ours. It has been this continuous sharing and communication of ideas over the years that has transformed my relationship to George Foster from that of student-apprentice into that of friend and colleague. Despite the long distances between Cairo and Berkeley and my own ethnographic shift from Mexico to the Middle East, George has never ceased to maintain a concerned and keen interest in the trajectory of my own career. On the one hand has been his demonstrated trust in my own professional judgments by his personalized attention in helping my students from the American University in Cairo be accepted and trained at Berkeley.

On the other hand is his ever perceptive and critical evaluation of the canons of honest research. In recent years I have been involved in trying to interpret the role of anthropology in primary health care for the World Health Organization, Eastern Mediterranean region. In 1976 I undertook my first consultancy assignment for this same organization as a technical adviser to study the Primary Health Care Programme in the Sudan. The purpose was to figure out ways and means of developing methodologies, materials and techniques on the training of village level primary health care workers in health education and health promotion. Knowing that Foster was also involved with similar activities, I wrote to him expressing the challenge and frustration at this new type of "research" experience. He honestly answered as follows:

I am delighted that you are being drawn into medical anthropology. The Sudan must have been fun. Yours seems a completely different branch from the one I have been involved in. Most of the people I worked with were in the Division of Strengthening Health Services. The group's report which is recently out, and worth very little, is based on *very* poor research, if you can call it that at all, then dressed up, and emerges as a very authoritative-looking document. Makes me suspicious of *all* such publications. Its purpose is not really research; it is to justify policy previously made—in this case Primary Health Care as the approach to health needs of developing countries (*Letter*, February 1, 1977).

Despite this forthright assessment of the pitfalls of international health services research Foster continues to be committed to the necessity of relating the insights of our discipline to the broader problems of the world. In the fall of 1978 he went to Manila and Kuala Lumpur to teach health educators about the social aspects of changing habits. And although he felt that formal public health education is pretty discouraging and that practitioners can't get it out of their head that "communication" is not all there is to getting people to practice more healthful living.

I struggle—I don't know how successfully—to try to put the idea across that to change people's habits, and to decide whether you ought to try to change them, you've first got to know what they believe and what they do. Seems like an obvious point, but it isn't (*Letter*, January 24, 1979).

Foster's entire academic career has been devoted to the mutual interplay and influence of theoretical and applied anthropology and I as well as the discipline are much more enriched by his efforts. As he recently wrote: "If we can't get anthropology out into the world, we will become a dying science. It is incredible how difficult it is to get some people off their thrones" (*Letter*, September 26, 1978).

As Foster is about to embark on a new stage of his long and illustrious career we may look back upon those "First Thirty Years" as ones of continuous enrichment and productivity. It is incumbent upon us, his students, to sustain the harvest that he has so assiduously cultivated. I, for one, look forward to the "Second Thirty Years" where his colleagueship and friendship may be more richly savored in the leisure of his retirement.

Happy Holidays 1999

For reasons that will become apparent it has not been possible to send a personalized letter this year. We, along with prescription drugs, are becoming more and more generic, and this is our first generic Christmas greeting.

This has been an interesting, and largely good year for us. We spent Christmas, 1998, in Mexico, with Melissa, Wijbrandt, and their youngest daughter, Klaartje, a student at the University of Amsterdam. We were in Tzintzuntzan – the village, we have studied since 1945 – for our eighth Christmas, always a satisfying experience.

Then, in April we flew to Holland for the wedding of Melissa's oldest daughter, Christy. We'll not say more than that the day was one for the books. Christy and her fiancé, Rob BuroW had rented Kasteel Wijenburg – a very homey little "castle" – near Nijmegen, where Melissa and family have lived for the past 15 years. The bride and groom arrived in a coach-and-four, complete with liveried footman and driver, to the huzzahs of family and friends. We lasted from noon until ten in the evening, and the party was still going strong when we left. Christy is successful as a psychologist working with disturbed school children, and Rob and his partner are doing well with a soft-ware company they have established.

Beginning in August, health problems have afflicted us. Six years ago George was diagnosed as having Parkinson's Disease. Until this year medication appeared to be controlling it. But then walking problems became severe, and he has fallen a number of times. To make a long story short, the doctors now believe he suffers from PSP (Progressive Supranuclear Palsy), a rare illness often misdiagnosed as Parkinson's. It is preferable to the latter in that he has no tremors, and less attractive in that there is no medication that brings significant relief, and walking becomes more and more difficult. Fortunately he can still drive. In September Mickie was diagnosed as having ovarian cancer. Following surgery she is undergoing chemotherapy, and we are optimistic about the outcome. Happily, her oncologist has cleared her to travel to Kauai where, months ago Jeremy and Angela had rented a beach-front house for Christmas week and invited us to join them, along with their daughters, Emily and Zoë. We leave on December 20. To cap off the good news, Christy and Rob are expecting a baby in early January – possibly on our 62nd wedding anniversary, Jan. 6!

So we have much for which to be thankful as we enter the new century. Our thoughts are with all of you, friends of many years, and family members. We wish you all a Merry Christmas and a Happy Millennium!

Oh, the picture. It was taken on October 24, 1999 at "Snag," our vacation pad in the Gold Rush country, in Calaveras County, three hours from Berkeley. Pay no attention to the date on the picture: it is the only one the camera prints, and it happens when least expected in spite of George's best efforts to shut the damned thing off.

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APPENDIX H

March 3, 1999

To: All Department of Anthropology Faculty

From: Laurie A. Wilkie
Nancy Scheper-Hughes
Kent Lightfoot

Re: Statement concerning the issue of Ishi's remains.

Attached you will find a draft of the statement prepared by the "Ishi's brain" committee. The content of the statement is intended to reflect the content of our discussions during Monday's faculty meeting. To ensure that we have not been misrepresentative, we would appreciate if each of you could critically review the statement and offer any comments or suggested revisions to us as soon as possible.

Laurie Wilkie will oversee the coordination of the comments and suggested revisions, so please direct your responses to her departmental mailbox.

DRAFT #1 - Accompanied March 3 memo of Wilkie, Schepher-Hughes
 Lifetfit

The recent recovery of a famous California Indian's brain from a Smithsonian warehouse has led the Department of Anthropology at the University of California Berkeley to revisit and contemplate a deeply regrettable part of this institution's history. Ishi, whose family and cultural group, the Yahi Indians, were murdered as part of the systematic genocide that characterized the influx of Europeans to California, lived his last years at the University of California, as an informant to one of our department's founding fathers, Alfred Kroeber. The nature of the relationships between Ishi and the anthropologists and linguists who worked with him at Berkeley's museum of anthropology were complex: friendships entwined with academic ambitions, and clearly, exploitation in the name of "science". Like millions of "first contact" native peoples, Ishi contracted tuberculosis, a disease introduced from the Old World. Ishi succumbed to the disease while Kroeber was in Europe. Ishi had trusted Kroeber to ensure the sanctity of his remains after his death, and Kroeber, although unintentionally, ultimately betrayed this trust, failing to stop an autopsy of Ishi's remains, and allowing his brain to be studied, and finally, curated, at the Smithsonian. Members of our Department had always assumed that the brain was cremated and buried with the rest of Ishi's remains in the Colma cemetery. The existence of this travesty is as shocking and surprising to our Department as it is to the rest of the public.

The world, the objects of our study, and the uses of anthropology have changed considerably since Kroeber's time. We now realize that the anthropology that emerged in the early 20th century—so called "salvage anthropology"—was in reality, a human science that emerged and grew up in the face of American genocide. Today, anthropologists are recognizing a new ethical position: to name and identify the sources, structures and institutions of violence against the indigenous people of America. It is the goal of anthropological pursuit to uphold the dignity and intrinsic value of human beings and the cultural diverse ways that they experience the world. Many of our faculty, through their research, attempt to illuminate the cultural processes and structures of inequality that lead to human suffering and oppression. We find abhorrent our department's role in our faculty's exploitation and betrayal of Ishi, a man who had already lost all that was dear to him at the hands of European colonizers.

Ishi was just one of the millions of Native American peoples whose lives were devastated by the horrors of European contact; he lends a face, a name, a personalized narrative to the vast genocide that occurred in this country. As such, Ishi has come to represent more than the life of a single man, coming to symbolize the broader experience of Native Americans during the conquest of the New World. Science proceeds by correcting past error and through a gradual process of critical self-reflection. What happened to Ishi, at the hands of science, was a desecration and a perversion of the larger values in anthropology that we hold most dear and certain. We, the Anthropology Department of the University of California, Berkeley, wish to express its profound distress, anger, and sorrow for what happened to this tragic human being.

We are pleased that the process has begun that will allow Ishi's remains to return to California, and that his remains will at last receive the dignity and respectful treatment they so deserve. This revisiting of a most regrettable chapter of our departmental past has initiated productive dialogues among our faculty regarding how we best learn from our past and work, through our research, our teaching and our programmatic directions to ensure that the peoples and cultures that we study are not harmed, exploited or appropriated by our research. As part of our discussions, we are considering ways to more visibly pay honor and respect to Ishi's memory, perhaps by lending his name to one of our departmental spaces, such as the currently unnamed building at 2251 College Ave., or possibly by renaming Kroeber Hall or the Anthropology Museum. We see public participation as a necessary component of these dialogues, and invite particularly the peoples of Native California, part of the public we have been created to educate, to instruct us as to how we may better serve the needs of their communities through our research related activities. To this end, we would like to invite California Indian tribes to participate in a symposium for the fall of 1999 to celebrate the legacy of Ishi and modern California Indian culture. Perhaps, working together, we can ensure that the new millennium represents a new era in the relationship between Indigenous peoples and anthropologists.

MEMORANDUM

TO: Nancy Scheper-Hughes
 FROM: George Foster
 RE: Your memorandum on the Ishi brain case
 Date: March 12, 1999

I appreciate your offer to consider my suggestions for modification of draft no. 3 on the Ishi brain debate. After thinking more about the problem I have decided that this is not the appropriate course of action.

I am fascinated by the way two anthropologists with comparable training and lots of field experience can, using essentially the same factual base, come to such different conclusions. If I understand correctly, you see Kroeber and the Anthropology Department's stance at the time of Ishi's death as a metaphor for the genocide inflicted on Native Americans by Europeans, and that consequently today's anthropologists, and the Department of Anthropology must, in the best mea culpa fashion, apologize to Native Americans.

While I agree that the genocide was a black mark on Euro-American "civilization." I feel it is wrong to hold Kroeber and his associates, and the Department, responsible for Ishi's death. Our differential readings of the record certainly support my belief that anthropology is the search for the most plausible interpretations of data rather than a question of testing and proving hypotheses.

If you will put a copy of your final draft in my box upon returning from New York I will probably write a commentary on it, pointing to the interpretations with which I disagree, and giving reasons for my views. This will be for internal Departmental distribution; I certainly have no thought of sending it to the media. I will add to the published record recollections Mickie and I have of conversations with Kroeber, and especially Edward and Delila Gifford, on the subject. It recently occurred to me that John Rowe, Gene Hammel, and Mickie and I are the only survivors in the department who have had the opportunity to hear first hand from some of the anthropologists involved in those far off times.

I first heard of Ishi in Kroeber's course on the Indians of California, taken shortly after I began graduate study in 1935 - just 20 years after Ishi's death. What struck me then, and continues to seem important, is the genuine emotion and sympathy for Ishi Kroeber felt, and how lucky Ishi was to fall out of the arms of a county sheriff and into the arms of anthropologists who, because of prior linguistic research on Yana, were probably the only people able to establish contact with him, begin to quiet his terror and convince him that not all white men were murderers.

Cc: Brandes, Lightfoot, Wilkie and several interested emeriti

(48)

MEMORANDUM
March 24, 1999

TO: Anthropology Faculty, UCB

FROM: George Foster

RE: March 17, 1999 "Statement on Ishi's Brain"

(NOTE: This memo, slightly revised on March 30, and again on May 3, addresses the drafts submitted to staff members up to and including the March 17 "Statement on, etc." It does not speak to the March 29 PRESS RELEASE faxed to PUBLIC AFFAIRS, UCBERKELEY.)

I was greatly distressed by the March 2 memo on "the issue of Ishi's remains," signed by Nancy, Laurie Wilkie and Kent Lightfoot. I felt that it did not accurately reflect the history of this affair, insofar as I knew it from Theodora Kroeber's two books on Ishi, her biography of Kroeber, and my recollection of discussions about Ishi with Kroeber, and especially Edward and Delila Gifford, all of whom talked often of the story, which was still recent history when I began graduate study in Berkeley in 1935.

My reaction is essentially the same as that expressed by Nelson Graburn in his March 8 reply to the same memo: "Thank you for your draft letter. I think it is better than the prior one, but I have some reservations. They may be summed up by what seems to be your attempt to make out everything as BAD as it possibly could be, instead of being more straightforward and balanced." (Quoted by permission of Dr. Graburn).

The reality of the genocide cannot - and should not - be denied: T. Kroeber's three chapters in Ishi in Two Worlds, pp.40-100 are as graphic a description of the systematic extermination of a people as can be imagined. This is a stain on the hands of all Americans of European descent (as is the treatment of Afro-Americans brought to this country as slaves, and their living descendents and, in lesser degree, other ethnic minorities as well). Yet to suggest that the University of California, and Alfred Kroeber and other anthropologists bear a special blame strikes me as untrue and unfair. The memo pulls out all the stops: through the use of loaded expressions such as "betrayed the trust," "the existence of this travesty," "we find abhorrent our department's role in our faculty's exploitation and betrayal of Ishi." it implies that the behavior of Kroeber and other anthropologists is a metaphor for the 19th century genocide of the California Indians.

After reading the memo I called Nancy and expressed my message conveyed was not only unfair to the ment and its anthropologists, but also potentially damaging to the University which, as we all recognize, has critics only too happy to denigrate the institution. I also expressed doubt that the memo represented the feelings of a majority of the members of the Department, and that I felt it would be more accurate to introduce the statement with something like "We, the undersigned members of the Department of Anthropology," etc. We had an open and amicable discussion of our points of view, in the course of which Nancy told me that a second draft had been drawn up, that I would find less extreme. This was indeed the case, and after reading it the following morning I drew up the attached memo (dated 3/12/99). My main thought was expressed as follows:

"I am fascinated by the way two anthropologists with comparable training and lots of field experience can, using essentially the same factual base, come to such different conclusions. If I understand correctly, you see Kroeber and the Anthropology Department's stance at the time of Ishi's death as a metaphor for the genocide inflicted on Native Americans by Europeans, and that consequently today's anthropologists, and the Department of Amthropology must, in the best mea culpa fashion, apologize to Native Americans.

"While I agree that the genocide was a black mark on Euro-American "civilization." I feel it is wrong to hold Kroeber and his associates, and the Department, responsible for Ishi's death. Our differential readings of the record certainly support my belief that anthropology is the search for the most plausible interpretations of data rather than a question of testing and proving hypotheses."

While trying to solve this conundrum I reread Nancy Curtius' Los Angeles Times (June 5) 1997 article in which she (correctly) points out that until Theodora Kroeber's Ishi in two worlds was published in 1961, Ishi had gradually faded from public consciousness. She then (incorrectly) quotes Ira Jacknis (Associate Research Anthropologist in the Hearst Museum) as saying

"There was a generation of anthropologists who just wanted to forget the embarrassment of an Indian living in a museum."

Ira tells me he believes this statement describes the feeling (which he does not share) of a good many contemporary anthropologists. If so, it casts light on how Nancy and I can construct such different pictures from the same data base. John and Patricia Rowe, Gene Hammel, and Mary Foster and I - all of whom were here well prior to 1961, agree there was absolutely

no such feeling. Rather, the few people who were aware of Ishi at all felt the treatment he had received at the hands of Kroeber, Waterman, Pope, and Gifford, was exemplary, an ethical credit to anthropology. I shared (and continue to share) this belief.

[The memo was addressed to Nancy, with copies to Lightfoot and Wilkie, and "several interested emeriti" whom I knew felt the original draft was overly strong. I also talked in person with Nancy following the George Marcus lecture in Room 100 in Boalt Hall. A day or two later Nancy's draft #3, arrived. This is a somewhat longer version of her draft #4, the final STATEMENT ON ISHI'S BRAIN, dated March 17. This is the statement that, signed by Departmental members who wish to do so, is intended for the media, as the Department's official stance on the matter].

[In my memo of March 12 I comment on the remarkable differences in interpretation of a common body of data that can characterize two anthropologists. Nancy speaks of the complex relationships between Ishi and the anthropologists while Ishi lived in the old Museum of Anthropology adjacent to the UCSF school of medicine: "real friendship entwined with academic ambitions, resulting in considerable insensitivity to Ishi's personal and medical needs." He contracted tuberculosis almost a year before he died (in 1916) "and while already quite ill, Ishi was sometimes overworked as a key informant." Although Kroeber was entrusted by Ishi to take proper care of his remains, he (Kroeber) "was unable to prevent an autopsy during which Ishi's brain was removed...to be shipped to and curated at the Smithsonian."]

Ishi did live in the museum, and he was the source of a great deal of invaluable ethnographic and linguistic information for Kroeber, Gifford, Waterman, Sapir, Saxton Pope and others. And he did contract tuberculosis, and during his last summer he worked hard with Sapir. As to whether Ishi felt he was being exploited or not, we will never know. My impression - colored no doubt by my relationship with Eben Tillotson, a Yuki Indian friend during my first field work, in Round Valley, California, during the summer of 1937 - is that Ishi was pleased to help make sure the record of his language and culture was as accurate as possible. Eben, who was literate, always insisted on looking in my notebook to make sure that I correctly recorded names and places, and other data as well. "What my grandchildren will know of how we Yuki lived will depend on what you write, and you must be very careful in recording what I tell you."

In the remaining paragraphs of this memo I speak to the main point in paragraph #4 of my March 12 memo, namely a commentary "pointing to the interpretations with which I disagree, and

giving reasons." Most of the factual data are drawn from Theodora Kroeber, Ishi in Two Worlds, 1961, and from Nancy Rockafeller's long memorandum dated 2/17/98, "A Compromise Between Science and Sentiment": A Report on Ishi's Treatment at the University of California, 1911-1916.

On August 29, 1911, sleeping butchers were awakened by dogs barking in the corral of the slaughter house where they worked, near Oroville, in the Sierra foothills. Investigation revealed a near-naked Indian, cowering against the corral fence. The butchers called the sheriff who, to protect the Indian, took him to the county jail, where speakers of several Indian languages tried in vain to communicate with him. The story of the "wild Indian" reached the San Francisco newspapers, where Alfred Kroeber and T.T. Waterman read it. They suspected the Indian was a speaker of Yana, two of whose four dialects had been studied and vocabularies recorded by anthropologists.

On August 31, Waterman went to Oroville where he ran through his Yana word lists with the Indian, in vain until he came to Siwini ("yellow pine"), which Ishi (as I will now call him) recognized. More words were recognized, and Waterman concluded that Ishi was Yahi, a speaker of the southern-most Yana dialect, all of whose speakers had been assumed to be dead. Batwi, one of two Yana who had worked as informants for anthropologists was still alive, living in Redding, a considerable distance north of Oroville. Batwi came to Oroville where, in spite of greater dialectical differences than expected, he succeeded in communicating with Ishi. With the permission of the Indian Bureau in Washington, on September 4 Waterman brought Ishi to the Museum of Anthropology in San Francisco, where he lived out the remaining four years and seven months of his life.

Recognizing the tragedy of Ishi's story, I have always thought how lucky he was to have had as his closest friends anthropologists who, on the basis of data gathered from earlier Indian informants, were able to communicate with him, recognize his personal and psychological needs, and make his last years better for him than if he had been sent to an Indian Reservation, the most probable alternative. In our discussions Nancy expressed the opinion that housing Ishi in a museum was degrading treatment. The museum, in addition to rooms filled with artifacts of California tribes, had a bedroom where Indians could stay when they came to San Francisco; the museum also had bathing and cooking facilities. Ishi differed from the other Indians in being a permanent resident. In addition to Indian guests there were two caretakers who always slept in the museum, as guards against fire. "So it was," writes T. Kroeber (Ishi in Two Worlds) pp. 123-124) "that when Ishi arrived, Indian guests were a normal part of museum living, and his own sense of

strangeness was made less acute through his new acquaintances' feeling of ease and accustomedness."

Not the least of the advantages of the Museum as home for Ishi was its location adjacent to the UCSF School of medicine. Saxton Pope, M.D. who joined the medical faculty a year after Ishi arrived, became fascinated by the latter's skill as archer and bow-maker. Ishi instructed Pope in the art and science of Yahi archery, and the two men became fast friends. "Popey" also became Ishi's personal physician, and it is unlikely that any San Franciscan had better, more constant and concerned medical care than Ishi. It is simply wrong to speak of "insensitivity to Ishi's personal and health needs."

Of those who criticize Kroeber's decision to arrange for Ishi to live in the Museum, I ask, "What would you have done? Left him with the Sheriff? Sent him off to an Indian reservation?" I am convinced that Ishi was better off in the Museum than he would have been any place else.

To me the most remarkable event of Ishi's last years was the month-long camping trip he made to his aboriginal home in May, 1914, with Popey, 11-year old Saxton, Jr., and Kroeber and Waterman. The attraction of the trip for Popey and his son was the excitement of serious bow-and-arrow hunting, as well as the romance of living like real Indians. For the two anthropologists the attraction of the trip lay in the unparalleled opportunity to see and photograph scenes of aboriginal life, until that time known only from informants' statements, now portrayed before their eyes. Initially opposed to the trip, Ishi quickly was caught up in the excitement of the occasion, and happily demonstrated to his friends many aspects of his former life. In the mid-1950s Mickie sat next to Saxton, Jr. at a dinner party. When he learned she was an anthropologist he told her many things about that trip, how he remembered crossing a swift stream, holding on to Ishi's long hair as Ishi swam with strong strokes, how he and his father hunted with Ishi with bow and arrows, and the like. He felt that Ishi was enjoying the experience almost as much as he was. Certainly, photographs in Ishi in Two Worlds often portray a happy person. After several weeks, however, he was anxious to return "home," to the museum.

I believe Theodora Kroeber's accounts are fair and accurate, insofar as data were known and available. Edward and Delila Gifford were the two people closely associated with Ishi whom Mickie and I knew best. As assistant museum curator "Gifford probably saw more of Ishi at least during museum hours than did the others [Kroeber, Waterman, Pope]; certain it was he [who] carried the brunt of responsibility for him during the last months of Ishi's life" (Two Worlds, p.153). From time to time

legal possessor before it was sent to Dr. Hrdlicka. If you will enter as the donor the Department of Anthropology of the University of California, I think your record will be as accurate as you can make it." In Rockefeller Report, Appendix H]. In view of the San Francisco Chronicle article this morning (March 24) reporting the Smithsonian's decision to return the brain ("Smithsonian to Send Ishi's Brain Home"), the question may be moot.

#4



Corresponde

Assuming Responsibility for Ishi

The Department of Anthropology, U of California Berkeley, in response to numerous inquiries from members of the profession, wishes to clarify for readers our considered opinion on the disposition of Ishi's brain. We reached general agreement on these words at a regular faculty meeting held March 29, 1999.

The recent recovery of a famous California Indian's brain from a Smithsonian

warehouse has led the Department of Anthropology at the U of California Berkeley to revisit and reflect on a troubling chapter of our history. Ishi, whose family and cultural group, the Yahi Indians, were murdered as part of the genocide that characterized the influx of western settlers to California, lived out his last years at the original museum of anthropology at the U of California, which was then located in San Francisco. He served as an informant to one of our department's founding members, Alfred Kroeber, as well as to other local and visiting anthropologists. The nature of the relationships between Ishi and the anthropologists and linguists who worked with him for some 5 years at the museum were complex and contradictory. Despite Kroeber's lifelong devotion to California Indians and his friendship with Ishi, he failed in his efforts to honor Ishi's wishes not to be autopsied and he inexplicably arranged for Ishi's brain to be shipped to and to be curated at the Smithsonian.

We acknowledge our department's role in what happened to Ishi, a man who had already lost all that was dear to him. We strongly urge that the process of returning Ishi's brain to appropriate Native American representatives be speedily accomplished.

We are considering various ways to pay honor and respect to Ishi's memory. We regard public participation as a necessary component of these discussions and in particular we invite the peoples of Native California to instruct us in how we may better serve the needs of their communities through our research related activities. Perhaps, working together, we can ensure that the next millennium will represent a new era in the relationship between indigenous peoples anthropologists, and the public.

Stanley Brandes
Chair, Department of Anthropology
UC Berkeley

[For further information on the subject of Ishi's brain, please see, "They Saved Ishi's Brain!" by J Marks in the April 1999 AN, p 22—Ed]



BY GEORGE M FOSTER
U OF CALIFORNIA, BERKELEY

Assuming Responsibility for Ishi (May 1999 AN, p 2) reports the formally expressed opinion of the UC Berkeley Anthropology Department that Alfred Kroeber and his colleagues treated Ishi (the Native American who appeared near naked and starving in a slaughter house corral in Oroville, CA in 1911) unethically, and that a formal apology is necessary. I believe the anthropologists' behavior was entirely ethical—judged by standards of the time—and that apologies are unnecessary. Many colleagues share this view, including all who knew Kroeber and the Giffords—the only people still living in the 1950s who knew Ishi well.

COMMENTARY

Recent Furor

The present furor over Ishi began with *Los Angeles Times* writer Mary Curtin's article, "Ishi: Group Seeks to Rebury Tribe's Last Survivor in Homeland" (June 8, 1997). When this article appeared the location of Ishi's brain was a mystery, but her statement that it had been held at UC San Francisco implicated the Medical School. UCSF medical historian Nancy Rockafeller's unpublished report, "Compromise Between Science and Sentiment: A Report on Ishi's Treatment at the University of California, 1911-1916," (February 17, 1999) recounts how a rumor led to anthropologist Orin Starn's (Duke U) verification in January 1999 that in 1917 Kroeber had shipped Ishi's brain to Aleš Hrdlička at the Smithsonian Institution. Following Hrdlička's retirement in the 1930s, the brain was essentially forgotten.

Starn's revelation that the Smithsonian held Ishi's brain led to a

rash of California newspaper articles demanding its return to Native Americans for reburial with his ashes. Most articles were temperate, arguing that in simple justice the brain *should* be returned. However, the *Contra Costa Times* and several UCB Anthropology Department memoranda used strong language to criticize the anthropologists who first communicated with Ishi and cared for him until

"failed in his efforts to honor Ishi's wishes not to be autopsied" (April 6, 1999).

The March 17, 1999 UC Berkeley document, "Statement by Members of the Department of Anthropology on the Proper Treatment of Ishi's Brain"—final version of several earlier drafts—struck a significant number of UCB active and emeritus professors as unfair both to the university and to Kroeber

In view of close association and friendship with several of the principles in this case, I respond to the most commonly voiced criticisms of Kroeber, his colleagues and the university.

Living Museum Exhibit

Kroeber and Waterman took control of Ishi, placing him in a museum, where he became a major exhibit for as long as he lived.



Alfred Kroeber photographing Ishi in the Deer Creek area of northern California, May 1914. Probably photographed by Saxton Pope.

Phoebe Apperson Hearst Museum of Anthropology and Regents of the U of California

his death: "This is the final indignity heaped upon Ishi by anthropologists . . . they made him a curiosity. They studied him like a rat in a cage . . . they carved him up and sent his brain to the Smithsonian" (March 26, 1999; emphasis added). *Oakland Tribune* writer William Brand wrote that the UC Berkeley anthropology department was "in a tizzy," expressing regret about the brain and accusing Kroeber of having

and his associates. This statement characterizes the relationship between Ishi and the anthropologists who worked with him for nearly five years as a "real friendship entwined with academic ambitions, resulting in considerable insensitivity to Ishi's personal and medical needs." An earlier draft includes the phrase "exploitation in the name of science" (March 3, 1999; emphases added). Further, the draft raises the specter of genocide and expresses the UCB Anthropology Department's "abhorrence" of its role in the exploitation and betrayal of Ishi, as well as its "profound distress, anger, and sorrow for what happened to this tragic human being."

Other living arrangements (eg. an Indian reservation) might have been more suitable.

Acknowledging the tragedy of Ishi's life, he was fortunate to have as his closest friends anthropologists who, from data offered by earlier Native American informants, were able to communicate with him, recognize his personal and psychological needs, and almost certainly make his last years more comfortable and satisfying than if he had been sent elsewhere. When viewed from the perspective of contemporary race relations and political correctness the decision to house Ishi in the museum carries unfortunate implications. Human beings live in hotels, boarding

Commentary Policy

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houses and homes. Specimens are stored, studied and exhibited in museums. Inevitably, housing Ishi in the museum suggested that he was stored there to facilitate his roles as informant and exhibit. Unquestionably Ishi attracted visitors to the museum when, on some Sunday afternoons he and Kroeber appeared together, Ishi demonstrating Yahi flint chipping and bow and arrow making, and Kroeber answering questions and translating for Ishi and museum visitors.

In reality Ishi's situation in no way conformed to this popular perception. In addition to housing artifacts of California tribes, the museum had living quarters for Native American visitors to San Francisco. Ishi differed from the others in becoming a permanent rather than transient resident. In addition to Native Americans, two guards against fire also slept in the museum.

Of those who criticize the decision to house Ishi in the Museum, one must ask, "What would you have done? Left him with the sheriff? Sent him to an Indian reservation, where health conditions almost certainly would have led to an early death?" I believe Ishi was happier and better cared for in the museum than he would have been any place else. Moreover, the Museum's location adjacent to the medical school was fortunate for Ishi's health. Saxton Pope, MD, became fascinated by the latter's skill as archer and bow-maker. Ishi instructed Pope in Yahi archery, and the two men became fast friends. Pope also became Ishi's personal physician, and it is unlikely that any San Franciscan had better, more constant and concerned medical care than Ishi.

Anthropological Exploitation
Anthropologists exploited Ishi, "pumping" him for information about his aboriginal life, thus hastening his death when he fatigued quickly because of the ravages of tuberculosis.

As to whether Ishi was exploited depends on how one defines the term. Ishi was indeed the source of invaluable ethnographic and linguistic information for Kroeber, Gifford, Waterman, Sapir and Pope. Did he feel exploited? We can never know. My impression—doubtless colored by my relationship with Eben Tillotson, a Yuki

Everyone has a right to expect that history's judgments be made against a background of total performance, and not on the basis of a single episode, evaluated by the standards of a later time.

friend during my first fieldwork in 1937—is that Ishi was pleased to help make the record of his language and culture as accurate and complete as possible. Tillotson, who was literate, frequently insisted on checking my notes: "What my grandchildren will know of how we Yuki lived depends on what you write, so you must be very careful in recording what I tell you." Because of the cooperative efforts of California Native Americans telling anthropologists about their way of life, and the publications that followed, all of us—Native American and non-Native American alike—today know much more about aboriginal California than we otherwise would.

Violation of a Sacred Trust

By failing to prevent an autopsy and sending his brain to the Smithsonian, Kroeber violated a sacred trust with Ishi.

With respect to "carving Ishi up and sending his brain to the Smithsonian," the following background is relevant. Kroeber was on sabbatical leave in Europe and New York City during Ishi's last illness. Realizing that Ishi was near death he wrote to Gifford, "Please stand by our contingently made outline of action, and insist on it as my personal wish . . . I do not see that an autopsy would lead to anything of consequence . . . If there is any talk about the interests of science, say for me that science can go to hell. We propose to stand by our friends" (T Kroeber 1961, page 234). Although the letter arrived too late to prevent an autopsy, it makes clear that Kroeber had strongly opposed such action before he departed on sabbatical leave eight months earlier.

Autopsy was standard practice for all patients who died in the hospital, but removal of the brain was not. The question thus remains of

why Kroeber, in view of his opposition to an autopsy, sent Ishi's brain to the Smithsonian almost ten months after his death. Here we can only speculate. Kroeber, after a year's absence, returned to find the predictable pile of items demanding attention. What he had not expected to find was the bottled brain of a close friend in his office. The fact that he did nothing for five months speaks to his fatigue and uncertainty. To Kroeber the physical remains would have seemed distinctly secondary to the memory of a friend. But what to do with the brain? At that time it was believed much could be learned from the study of human brains. Kroeber knew that Hrdlička had a collection of primate brains. To add Ishi's brain to this collection must have appeared to be the best solution to this vexing problem: Ishi's last gift to science was his own brain! So in January 1917, Kroeber shipped the brain to Washington where, following Hrdlička's retirement it was forgotten for more than 60 years, until changing social conditions and concepts of justice made relevant the question of its location and control.

Over Sentimentality

Theodora Kroeber's account of Ishi's life is overly-sentimental, and inevitably insensitive to Ishi's view of his role in the drama. Never having known Ishi herself, she relied on her husband who, by her own account, was reluctant to discuss Ishi.

To this charge, one can only respond that whatever the shortcomings of her accounts, they constitute the basic source for everyone who writes on Ishi. I find her books fair and accurate. In the 1950s Edward and Delila Gifford often talked to my wife Mary and me about Ishi's weekend visits to their home, telling of his obvious pleasure at being in their company.

Their accounts of Ishi's life in the museum, and of his personality and behavior fully substantiate T Kroeber's portrayal.

The reality of genocide cannot—and should not—be denied. T Kroeber's three chapters (1961, pages 40-100), are as graphic a description of the systematic extermination of a people as can be imagined. This is a stain on the hands of all Americans of European descent. But to imply, as do the UC Berkeley memoranda, that the behavior of Alfred Kroeber and other anthropologists is a metaphor for the 19th century genocide of California Native Americans is ludicrous.

Everyone has a right to expect that history's judgments be made against a background of total performance, and not on the basis of a single episode, evaluated by the standards of a later time. In my view the documents discussed in this commentary deny Kroeber this right. Those who knew him well have not the slightest doubt as to his genuine affection for those Native Americans with whom he worked closely, as well as for them as a people. ■

*George M Foster is Professor Emeritus of Anthropology at the U of California, Berkeley, where he took his PhD in 1941. He first learned about Ishi in 1936 in Kroeber's *Indians of California* course. First as a student, and from 1953 until their deaths as a colleague, he was a close friend of Kroeber and Gifford, and of their wives, "Krakey" and Delila. His first field research was among the Yuki in Round Valley, CA, during the summer of 1937. Foster's subsequent research interests have largely concerned Mexico, especially his 54-year study of Tzintzuntzan.*



#6

Correspondence

Continued from page 3

Eating the Dead

Over much of its history, a strong element of anthropology has been its compassion for those it studies. That compassion has not been perfect. Anthropological approaches and techniques have been used in quite uncompassionate ways, ways that have been regarded by most of the field, most of the time, as deplorable. On the whole, anthropologists have been the defenders and protectors of their hosts, most of the time. I think of Kroeber and Gifford as Ishi's protectors.

What is most distressing in the controversy about Ishi (see May 1999 *AN*, p 2; and October 1999 *AN*, p 5) is the fury of the attack on anthropologists who, as best they could, and as fallible as they might have been, extended this compassion. What is distressing is the intolerance of failure to exhibit political perfection in the past by current definition, the disregard of historical context, the refusal to see humanity even in what they regard as failure.

I did not know Saxton Pope. I knew Gifford, who introduced me to his Pomo informants, with whom I worked as an undergraduate and who recalled him with affection. I worked for Gifford as a preparator in the UC Museum. He was a man of such kindness that at his retirement, and later at his death, people openly wept. I knew Kroeber less well—somewhat stern, sometimes irascible, but kind to an undergraduate and by no account inhumane. I knew Juan Dolores, a Papago, with whom I worked at the Museum of Anthropology, and who, like Ishi, lived on the premises much of the time. Other informants also worked in the Museum, for example Kanmo Imamura, who became well established as a religious leader in the Berkeley Japanese community. Kindness and respect were hallmarks of actions toward them.

As I think about those people, almost a half century ago, I cannot fathom the ignorance, intolerance and ahistoricity of those who attack Kroeber and Gifford, nor can I fail to remark on their classic attempts to gain status by eating the dead.

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INTERVIEWS ON THE HISTORY OF THE UNIVERSITY OF CALIFORNIA

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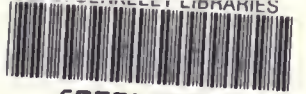
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